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PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

VOL. III.

MDCCCXLVIII—MDCCCLV.

STANFORD

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MDCCCLV.

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PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

FORTY-SEVENTH SESSION.

1*st November*, 1848.—*The* PRESIDENT *in the Chair.*

THE Librarian laid on the table a copy of the printed proceedings of the Liverpool Literary and Philosophical Society, No. 4, presented by that Society.

The President read a paper on the Atmosphere, explaining its nature and extent, and showing the maximum and minimum points of barometric pressure, and the varieties of temperature in different parts of the world. It was noticed that the place most remarkable for its climate is Yakutsk, (N. Lat. 62°,) which is near the boundary between Russian Siberia and Chinese Tartary, and where a considerable trade is carried on between these great nations. The thermometer in winter sinks as low as —77°. During three months of the year the temperature is never higher than 40°, and during three only is the temperature above the freezing point. These three months constitute the summer at Yakutsk. Snow and ice disappear all at once; the thermometer rises to 82°; crops of wheat are raised;—the sowing, the vegetation, the ripening, and the harvest are all completed before the brief summer terminates. It is well known that for the ripening of wheat, barley, rice, and other kinds of grain, a certain temperature is necessary. Thus, barley will not grow unless the mean temperature of the three summer months amounts to 49½°; wheat, which is a native of a hotter climate, requires a still higher temperature. But when the temperature is 82° at Yakutsk, there being a sufficient supply of water, the process of vegetation is remarkably shortened.

15*th November*, 1848.—*The* VICE-PRESIDENT *in the Chair.*

A COMMUNICATION was received from the secretary of the Liverpool Literary *and Philosophical* Society, intimating the additional gift of Nos.

1, 2, and 3 of that Society's proceedings. The thanks of the Society were voted.

Mr. Burgess exhibited and described his newly invented Vacuum Pump, for which he has received the Coulter premium; the successful competitor for the prize being required to exhibit his invention to the Philosophical Society, in terms of the following report and minute of Coulter's Trustees:—

"We most respectfully suggest to the Trustees that, as the premium mortified by Mr. Coulter was for the public benefit, the successful candidate should, on this and all future occasions, be required to exhibit and describe the invention at the earliest practicable meeting of the Glasgow Philosophical Society.—Signed, Andrew Liddell, Patrick M'Naught. Glasgow, 25th June, 1847."

"Which report, having been considered, the Trustees unanimously approve thereof, and direct the chamberlain to pay the premium of nine pounds sterling to Mr. Angus M'Kinnon, with a request that he would exhibit and describe the invention at the earliest possible meeting of the Glasgow Philosophical Society. And further, the Trustees recommend to their successors in office, that the suggestions made by Bailies Liddell and M'Naught, in the latter part of this report, should be followed by them in awarding future premiums for inventions.—Signed, Alexander Hastie."

Mr. Harvey exhibited specimens of leather belts and thongs cut by Mr. Foster's patent machine, also combination of thongs into ropes suited for the tillers of vessels. Cords of this construction are now also employed for shuttle cords in power-loom manufactories. Mr. Foster's machine is capable of cutting 1000 yards per hour. Mr. W. M. Buchanan described the machine, and mentioned that tiller ropes could be produced by this process at half the cost of those commonly in use.

The Society proceeded to the annual election of office-bearers, when the following were elected:—

President.

DR. THOMAS THOMSON.

VICE-PRESIDENT,..WALTER CRUM. | LIBRARIAN,...R. D. THOMSON, M.D.
TREASURER,.....ANDREW LIDDELL. |

Secretaries.

ALEXANDER HASTIE, M.P. | WILLIAM KEDDIE.

Council.

A. ANDERSON, M.D.	PROFESSOR GORDON.	JOHN STENHOUSE.
G. A. WALKER ARNOTT, LL.D.	WM. GOURLIE.	PROF. WM. THOMSON.
A. BUCHANAN, M.D.	ALEX. HARVEY.	GEORGE WATSON.
J. FINDLAY, M.D.	WILLIAM MURRAY.	A. K. YOUNG, M.D.



LIST OF MEMBERS.

ORIGINAL MEMBERS.

1815, Feb. 27, James Lumsden.
1819, May 24, Andrew Liddell.
1820, Feb. 21, John Hart.
" " " Robert Hart.
" June 12, Nicol Handyside.
" Aug. 21, John Herbertson.
1821, Jan. 22, Peter Aitken.
" July 16, John Ure.
1822, Oct. 14, John Stewart.
1827, Jan. 7, James Eadie.
" March 5, Alex. Hastie, M.P.
1831, Jan. 17, George Smith.
1834, Feb. 10, And. Buchanan, M.D.

ORDINARY MEMBERS.

1834, March 10, Richard S. Cunliffe.
" " " Daniel MacKain.
" April 7, John Joseph Griffin.
" " " Alexander Harvey.
" May 19, Walter Crum.
" Nov. 12, Thos. Thomson, M.D.
1835, Jan. 21, James Davidson.
" March 4, Robt. M'Gregor, M.D.
" April 1, John White.
" " 5, John Houldsworth.
" May 13, John Baird.
1836, Feb. 10, James B. Neilson.
" March 9, Thomas Dawson.
" Nov. 16, Graham Hutchison.
" " " C. Randolph.
" " 30, James Lumsden, jun.
" " " John Tennent, Bonnington.
1837, Nov. 15, John Stenhouse.
" Dec. 27, William Murray.
1838, Feb. 7, Jas. Smith, Deanston.
" " " James Murray.
" " 21, John Smith.
" March 21, John Black.
" " " Alexander Graham.
" Dec. 5, George Lancaster.
1839, March 27, William Wilson.
" Nov. 6, John M'Bride.
" " " William M'Bride.
" " 20, Fred. Penny, Ph.D.
" Dec. 18, Alex. G. Edington.
1840, Jan. 8, Alex. Wingate.
" " " George Robb.
" " 22, William Gourlie.
" Feb. 19, John Findlay, M.D.
" " 29, Fred. Adamson.
" *April 15, Matthew P. Bell.*
1840, April, 29, Wm. M. Buchanan.
" Dec. 2, William Cockey.
" " " L.D.B. Gordon, Professor.
1841, Jan. 27, J. Wilson, Auchineaden
" Nov. 17, And. Anderson, M.D.
" " " W. G. Blackie, Ph.D.
" " " Charles Glassford.
" " " William King.
" " " Archibald Walker.
" " " And. Kerr Young, M.D.
" Dec. 1, William More.
" " " William Ramsay.
" " " R. D. Thomson, M.D.
" " " James Thomson.
" " 15, J. G. Fleming, M.D.
" " " William Low.
" " " John Clugston.
1842, Jan. 26, George Thorburn, jun.
" March 8, Charles T. Dunlop.
" April 6, Wm. Hutcheson, M.D.
" Nov. 16, Hugh Colquhoun, M.D.
" " 30, William Spens.
" " " James Couper, Caledonian Pottery.
" Dec. 14, Andrew Stein.
1843, Jan. 4, Peter M'Intosh.
" " " George Sutherland.
" " " Thomas Hill.
" " 18, And. Quinlan, M.D.
" " " Alex. Mitchell.
" " " Andrew Mitchell, jun.
" " " Robert Graham.
" " " Henry Wardrop.
" " " Thomas Kyle.
" " " John Tennant.
" " " Charles James Tennant
" Feb. 1, William Craig.
" " " William Gale.
" " " William Strang.
" " 15, John Heugh.
" " " George Wilson.
" March 15, William Keddie.
" " " John M'Andrew.
" " 29, F. H. Thomson, M.D.
" Nov. 15, John Turnbull.
" " " Thomas Edington.
" " " John Fisher.
" " 29, Adam Patterson.
" " " George Jasper Lyon.
" " " Robert Balloch.
" " " James Johnston.
" " " James Bell.

1843, Nov. 29, Andrew Bain.
" Dec. 13, Charles Griffin.
" " 31, Walter Neilson.
" " " James Bogle.
" " 20, Robert Stewart.
" " " Alexander Grant.
1844, Jan. 17, John Morgan.
" " 31, William Smith.
" " " William Wilson.
" Feb. 28, Alexander M'Nab.
" March 13, William Crichton.
" " " Rev. John Graham.
" " " William Bankier.
" " " John Miller.
" Dec. 4, Alex. Warren Buttery.
" " " James Allan, sen.
" " " George Thomson.
" " " Matthew Fairlie.
" " " S. P. Cohen.
" " " William Gilmour, jun.
" " 18, Laurence Hill, jun.
" " " Thomas Watson, M.D.
" " " Alexander Wilson.
" " " Oliver G. Adamson.
1845, Jan. 8, John A. Easton, M.D.
" " " William Brown.
" " " Thos. G. Buchanan.
" " " Geo. S. Buchanan.
" " " James Reid Stewart.
" " 22, Robert Barclay.
" Feb. 5, John S. Miller.
" " " James Caldwell.
" " " William Gardner.
" " 19, James Stevenson.
" " " James P. Hamilton.
" March 5, Jas. Murray, Garnkirk.
" " 19, James Couper, Insurance Broker.
" " " Robert Freeland, Gryffe Castle.
" April 2, Robert Salmond.
" " " W. G. Mitchell.
" " 16, John Thomson, Annfield.
" " " David Chambers.
" Nov. 19, Alfred Hall, M.D.
" Dec. 3, George Harvey.
" " " Andrew Risk.
" " " Moses Hunter.
" " " J. A. Hutchison.
" " " James Shanks.
" " " David Cunningham.
" " 17, William Ambrose.
1846, Jan. 7, James Thomson.
" " " George Brown.

1846, Feb. 18, John Crawford, M.D.
" March 4, G. A. Walker Arnott, LL.D.
" " 23, George Mitchell.
" " " William Somerville.
" Nov. 18, John Finlay.
" " " Alexander Miller.
" " " Francis Liesching.
" " " John Carrick.
" " " Hugh Carswell.
" Dec. 2, Wm. Thomson, Professor.
" " " James Bryce.
" " " Thomas Callender.
" " " Robert Wylie.
" " " George Buchanan.
" " 16, Arch. B. Harley.
" " " Robt. Johnston.
" " " Hugh Bartholomew.
" " " John Erskine.
" " " John M'Haffie.
" " " John Houston.
" " " James Clark.
" " 30, Alexander Laing.
" " " Robert Laird.
" " " W. Brown.
" " " William Geddes.
" " " J. Young.
" " " Charles Robb.
" " " James M'Connell.
1847, Jan. 13, Thomas Macmicking.
" " " James Harvey.
" " 27, Robert Blackie.
" " " Henry M'Manus.
" " " J. M'Gregor M'Intosh.
" " " David Laidlaw.
" " " Patrick Robertson.
" " " John M'Dowall.
" " " Alexander Ferguson.
" Feb. 10, Donald Campbell.
" " " Hugh M'Pherson.
" " " John Fyfe.
" March 10, Charles Watson.
" " 31, Peter Stewart, M.D.
1848, Jan. 5, James King.
" " " George Robins Booth.
" " " Andrew Fergus.
" " " John Moffat.
" " 19, John Knox.
" " " John Smith.
" Feb. 2, John Macadam.
" " " John Barclay.
" " " William Watt.
" " 16, James Howe M'Clure.
" " " John Craig.



1848, March	1,	Charles R. Collins.	
" "	"	Thomas L. Patterson.	
" "	"	William Ker.	
" "	"	David Burgess.	
" April	12,	William Clark.	
" "	26,	William Connell.	
" Nov.	1,	James Wylson.	
" "	"	Alex. King, M.D.	
" "	"	Henry Kerr.	
" "	29,	James Anderson, Lord Provost.	
" "	"	John M. Rowan.	
" "	"	Gavin Rae, jun.	
" "	"	Robert Readman.	
" "	"	John F. Sloane.	
" "	"	George M'Leod, M.D.	
" "	"	James Beith.	
" Dec.	13,	Dugald Bannatyne.	
" "	"	Michael Connal.	
1848, Dec.	13,	James A. Campbell.	
" "	"	John Elder.	
" "	"	William Ferguson.	
" "	"	Robert M'Laren.	
" "	"	George Paterson.	
" "	"	Neil Robson.	
1849, Jan.	3,	David Y. Stewart.	
" "	"	William Johnson.	
" "	"	James Stevens.	
" "	17,	John Buchanan.	
" "	"	John Napier.	
" "	31,	James Jeffray, M.D.	
" "	"	John Jeffray.	
" "	"	Andrew Stein.	
" "	"	Andrew Laughlen.	
" "	"	Robert Anderson.	
" Feb.	14,	James Patterson.	
" March	28,	Robert Sinclair.	
" April	11,	Howard Bowser.	

The Treasurer presented an abstract of his account for Session 1847–48.

1847.	Dr.				
Nov. 11.—To Cash in Bank at beginning of Session,.....	£119	15	2		
1848.					
Nov. 11. — Interest on do.,.....	2	7	6		
				£122	2 8
To 18 Entries of New Members, at 21s. each,.....				18	18 0
— 15 Annual Payments from Original Members, at 5s.,.....				3	15 0
— 205 Annual Payments, at 15s.,.....				153	15 0
— Arrears from four Members, at 15s. each,.....				3	0 0
				£301	10 8
1848.	Cr.				
Nov. 11.—By Balance due Treasurer,.....				£2	0 0
— New Books and Binding,.....				143	4 1
— Printing Transactions, Circulars, &c.,.....				25	0 0
— Printing and Parchment for New Diplomas,.....				18	0 0
— Two New Book Cases,.....				24	0 0
— Rent of Hall,.....				15	0 0
— Coffee and Gas for Evening Meetings,.....				11	10 0
— Fire Insurance,.....				2	16 0
— Society's Officer and Poundage Collecting Dues,				9	8 6
— Postages, Delivering Letters, &c.,.....				12	12 2
— Grant to Botanical Section, ...				5	0 0
— Balance in Union Bank,.....				32	4 6
— — Provident Bank,.....				0	15 5
				£301	10 8

GLASGOW, 1*st November*, 1848.—We have examined the Treasurer's Account, and compared the same with the Vouchers, and find that there is in the Union Bank of Scotland Thirty-two Pounds Four Shillings and Sixpence, and in the Provident Bank, Fifteen Shillings and Fivepence, together, Thirty-two Pounds Nineteen Shillings and Elevenpence at the Socity's credit.

THOS. DAWSON.
WILLIAM COCKEY.

Note by Treasurer.—11*th November*, 1848.—The Balance at credit of the Society is £86 15s. 3d. less at commencement of this Session than it was at the same period of last year. This arises chiefly from an excess of expenditure to about that amount this year in the Library department, for Books, Binding, and Book Presses. There are six Members in arrears of payment for one year only each. At the commencement of last Session there were 213 Members on the roll, and during the sitting of the Session 18 Members were admitted, and 3 Non-Residents restored, making 234; but from this fall to be deducted two removed and non-resident, six resigned, and four dead, which reduces the number on the list, and from which dues are leviable, to 222.

The Exhibition Fund, deposited with the Corporation of the City, and Interest thereon to this date, amounts to £493 14s. 11d.

29*th November*, 1848.—*The* PRESIDENT *in the Chair.*

MR. KEDDIE reported that the Botanical Section had re-elected its office-bearers, as follows:—Dr. G. A. Walker Arnott, President; Mr. William Gourlie, Vice-President; Mr. Francis Leeshing, Curator of Herbarium; Mr. William Keddie, Secretary.

The following gentlemen were admitted members of the Society:—Mr. James Wylson, Alexander King, M.D., Mr. Henry Kerr.

Dr. R. D. Thomson read an account of the Thibet expedition under Dr. Thomas Thomson, Jun., which has been published in Sir William Hooker's Journal of Botany.

Dr. Walker Arnott brought under the notice of the Society a substance which has been lately imported into Glasgow, under the name of Piassava, or Piaçaba, but which, from its being unknown in the market, had to be again shipped for London, where, as well as in Manchester, it is well known as a substitute for bristles or whalebone for brushes and street besoms, besides being applied to many other purposes for which it is fitted, by the great length, elasticity, and strength of its fibre, as well as its capability of resisting the action of damp. In Brazil, it is made into strong ropes for ships and draw-wells, and is exported chiefly from Bahia. Dr. Arnott mentioned that it was obtained from the palm called now Attalea funifera by authors; and the portion used is understood to be the spatha, and the dilated base of the leaf-stalks, which latter splits into shreds, and hangs like a veil from the tree. The nut of this palm is figured by Gaertner, and was formerly known under the name of Cocos lapidea, and is imported under various names from Bahia and Brazil, for the purpose of being turned into umbrella handles, ink *bottles, and other* ornamental articles; but the importers of the nut

seem to have no idea of its relation to the Piaçaba palm. [A reduced coloured figure of this Palm has been published in Hooker's Journal of Botany and Kew Miscellany, for April, 1849.]

13th December, 1848.—*The* PRESIDENT *in the Chair.*

ON the motion of Mr. Cockey, it was agreed to print a supplementary catalogue of the Library.

The following gentlemen were admitted members of the Society:—Hon. James Anderson, Lord Provost, Messrs. John M. Rowan, John Fullerton Sloane, Gavin Rae, Jun., Robert Readman, and George M'Leod, M.D.

The following paper was communicated by Dr. R. D. Thomson:—

I.—*On Two New Salts of Chromic Acid.* By ARCHIBALD DUNCAN, JUN., ESQ.

IN 1827, Dr. Thomson described in his paper on the compounds of chromium, (Phil. Trans. 1827, p. 223,) the double salt—*Potash chromate of magnesia*—(KO CrO_3 MgO CrO_3 2 HO) obtained by digesting a solution of bi-chromate of potash over carbonate of magnesia. I obtained a corresponding lime salt about two years ago by the following process.—A boiling solution of bi-chromate of potash was poured over newly slaked lime in a tall vessel. The undissolved lime having subsided, the supernatant fluid, which was of a lemon yellow colour, was drawn off by a syphon, and slowly evaporated in a hot air stove at 80°. During the first two days of the evaporation, crystalline crusts of an *orange* salt were formed on the surface of the liquor, and required to be frequently removed. After this time, however, these crusts ceased to be produced, and crystals of a *yellow* salt began to make their appearance at the bottom of the evaporating bason, and in two or three days more a mass of beautiful crystals was obtained. The proportion of the orange to the yellow salt depends a good deal on the temperature employed in the evaporation. In one experiment the heat was raised to boiling, and no yellow crystals were obtained at all,—orange crusts continuing to separate as fast as they could be removed.

Yellow Potash Chromate of Lime.—This salt crystallises in lemon yellow four-sided oblique prisms. It is soluble in water, but insoluble in cold alcohol, and is formed in the latter part of the process described.

The salt, when ignited, fuses, and on cooling, the mass has a crystalline aspect, and is quite soluble in water.

The mean of several analyses gave the following result:—

	Experiment.	Calculation.
Chromic acid,	51·840	52·52
Potash,	23·900	24·24
Lime,	14·950	14·14
Water,	9·600	9·10
	100·290	100·

This corresponds nearly with the formula KO CrO_3 CaO CrO_3 + 2 HO. The water being slightly in excess. It, therefore, is a parallel compound to the magnesian salt described by Dr. Thomson.

Orange Potash Chromate of Lime.—The mode of formation of this salt has been already described. It is soluble in water. The mean of three analyses yielded the following result:—

Chromic acid,	52·070
Lime,	23·990
Potash,	17·550
Water,	6·230
	99·840

This approaches nearly to the formula 3 KO 7 CaO 7 CrO_3 5 HO. When this salt is ignited it does not fuse, and when cool its colour is yellow. It does not again altogether dissolve in water, and thence it appears to have undergone decomposition.

II.—*Notice regarding the Measurements of Heights, by Means of the Boiling Point of Water.* By G. A. WALKER ARNOTT, LL.D., *Regius Professor of Botany.*

AT the last meeting of this Society, a paragraph was read from one of Dr. Thomas Thomson's (jun.) letters from the mountains of Northern India, to the following effect:—

"Time, 6½ p.m., temperature of air 80°, water boiled at 203·6. By the way, 545 feet for a degree is too little, at least for considerable heights; nor will a constant multiplier answer at all. Professor Forbes has some number near 560 [*it is only* 549½,] for a constant multiplier, but that was found gave quite erroneous results at great heights. There remains still something to be done in the matter. What the thermometer really gives, in fact, is the pressure of the atmosphere, and its indications vary with that."

While making some unpremeditated remarks to the meeting on Dr. Thomson's interesting letters, I mentioned that no one doubted that the thermometer merely indicated the pressure of the air; but that its variations *were not* proportional to the difference of pressure, but to the

difference of the logarithms of the pressure,—that is, they were proportional to the differences of the approximate heights obtained by means of a barometer; that a constant multiplier *must*, therefore, give the *approximate* height; but that it would appear that Dr. Thomson had not applied the very important corrections for the temperature of the air to that approximate height, and which are as necessary for the thermometer, as when the barometer is used. At the same time I pointed out, that although the discovery of the above law, and the application of a constant multiplier, was now usually ascribed to Professor Forbes of Edinburgh, both were, at least twenty-five years previously, adopted by the late Sir John Leslie, in the Supplement to the 5th edition of the Encyclopædia Britannica, article Barometrical Measurements, which article was afterwards inserted in the 7th edition of that work; the former was published in the year 1817 or 1818, the latter about 1830. Sir John Leslie also taught the same method in the Natural Philosophy class,—at least he did so during the winter 1816–17, when he undertook the management of that class during the absence of Professor Playfair in Italy, and illustrated it by many curious examples which have not been printed, but which are referred to in my notes, taken while attending his lectures that winter. Professor Forbes says, (Trans. of Roy. Soc. of Edinburgh, xv. p. 413,) "It is singular that so elegant and so simple a result should have escaped every writer on the subject, (*so far as I know,*) even Deluc himself, who proposed the logarithmic law, and Wollaston who, unawares, adopted the true law as a first approximation, and then took a wrong one:" from which it would appear that he had never read his predecessor's article on the subject, or that at the time it had escaped his recollection. Professor Forbes arrives at his conclusion by means of some very valuable observations made by himself in Switzerland,—Leslie considered it as already established by Saussure's observations and Deluc's logarithmic law.

As Sir John Leslie's views seem to be little known, or forgotten, I shall here insert them:—

"The heat at which water boils or passes into the form of steam, depends on the weight of the superincumbent atmosphere. By diminishing this pressure, the point of ebullition is always lowered. It appears that while the boiling point sinks by equal differences, the corresponding atmospheric pressure decreases exactly, or at least extremely nearly in a geometrical progression: it being found that every time such pressure is reduced to one-half, the temperature of boiling water suffers a regular diminution of about eighteen centesimal degrees. This beautiful relation assimilates with the law which connects the density and elevation of the successive strata of the atmosphere. *The interval noticed between the boiling points at two distinct stations must be proportional to their difference of altitude above the level of the sea.* We have therefore only to determine the co-efficient or constant multiplier, which may be discovered, either from an experiment under the rarified receiver of an air pump, or *from an actual observation* performed at the bottom and at the top of

some lofty mountain. We shall prefer at present the observation made by Saussure on the summit of Mont Blanc. This diligent philosopher found, by means of a very delicate thermometer constructed on purpose, that water which boiled at 101°·62 in the plain below when the barometer stood at 30·534 English inches, boiled at 86°·24 on the top of that mountain, while the barometer had sunk to 17·136. Wherefore, the distance between the points of ebullition, or 15°·38 centesimal degrees, must correspond to an approximate elevation of 15,050 feet; which gives 978½ feet of ascent for each degree, supposing the mean temperature of the atmospheric column to be that of congellation. But it will be more convenient to assume 1000 feet for the constant multiplier, which corresponds to the temperature of 5½°."

In order to understand this last clause, we must bear in mind that Leslie directs us in barometrical measurements to multiply the approximate height by twice the sum of the centesimal degrees shown by the thermometer indicating the temperature of the external air; the product, with the decimal point shifted three places to the left, gives the correction to be added to the approximate height. So, after establishing that 978·5 feet corresponds to a difference of 1° Cent. in the boiling point, the atmosphere being supposed to be at freezing or 0° at both stations, he changes the multiplier to 1000, and finds the new medium temperature corresponding; this, from what I have said, will be $\frac{1000-978{\cdot}5}{{\cdot}9785} = 22$ nearly; and one-fourth of this, or 5½, is the result, as stated by Leslie.

It is remarkable that Professor Forbes also refers to one of the same observations made by Saussure, in order to prove his constant multiplier obtained empirically. This is 549½ for Fahrenheit's thermometer at the temperature of freezing, which gives 989·1 as the multiplier for each centesimal degree at the mean temperature of 0° Cent., or 1000 at a medium temperature of 2¼° Cent. There is thus a difference on the approximate heights of eleven feet in a thousand from this source alone—Mr. Forbes' multiplier making it so much more than Leslie's: and although this is of no great consequence, it becomes important to have the means of reducing it, or ascertaining the cause of the difference.

Mr. Forbes states, that Saussure's thermometer boiled at 212° Fahr., or 100° Cent., when the barometer stood at 28·777 English inches. There must be either some slight error in this, or in the other observations made by Saussure, and depended on by Leslie. And here let us take Deluc's formula, where m Log. $p + n = b$ the temperature of the boiling point, p being the barometric pressure: now if we make two observations by the same instruments, and call the pressures P, p, and the boiling points B, b, the difference is m (Log. P $-$ Log. p) $=$ B $-$ b: hence $m = \frac{\text{B} - b}{\text{Log. P} - \text{Log. } p.}$, and $n = \text{B} - \frac{\text{B} - b}{\text{Log. P} - \text{Log. } p.} \times \text{Log. P}$. Thus, we readily find the *co-efficient m, and the* constant n, by means of two observations made by

the same barometer and thermometer. If we assume as correct, the observations quoted by Leslie, then $m = 61{\cdot}30563$, $n = 10{\cdot}5944$, whence $61{\cdot}306$ Log. $p + 10{\cdot}594 =$ temperature of the boiling point on the Centigrade thermometer. But by this formula the barometric pressure $28{\cdot}777$ gives the boiling point equal to $100°{\cdot}03$ Cent., or $212°{\cdot}056$ Fahr., in place of 100° and 212° Fahr. On the other hand, if we assume that the thermometer showed 100° under the pressure of $28{\cdot}777$, and $86{\cdot}241$ under the pressure $17{\cdot}133$, as stated by Mr. Forbes, the formula becomes $61{\cdot}0935$ Log. $p + 10{\cdot}862 = b$, which gives, at the pressure of $30{\cdot}534$, $b = 101{\cdot}57$, instead of $101{\cdot}62$, mentioned by Leslie, as having been observed. The difference is $0°{\cdot}05$ Cent., or $0°{\cdot}09$ Fahr. Hence there is some error of observation, which, although trivial, must affect considerably any co-efficients obtained.

When a thermometer does not exhibit the boiling point of 100°, under the pressure of thirty inches, it is customary to reduce it to that standard by adding or subtracting the *same difference* from all the observations made by it. This is obviously incorrect; for the difference at the boiling point of 100° can only be got by multiplying the difference at some other point by 100, and dividing by what the thermometer does indicate at that pressure. It appears to me, therefore, preferable to derive all the co-efficients by means of the same instruments, and afterwards reduce them in the way just mentioned. Not only does every thermometer require a co-efficient and constant multiplier for itself, so as to make the actual boiling points correspond with those calculated from the barometer, but the co-efficient or constant multiplier of the difference of the boiling points by the same instrument requires to be adapted to the mode of calculation followed for ascertaining the approximate heights by a barometer. Thus, let the barometer stand at 30, and $17{\cdot}133$ inches respectively, according to Professor Forbes; "by Galbraith's tables,

For 30 inches,	29228
— $17{\cdot}133$ do	14593
Difference,	14635,"

whereas, by the more usual method, and that adopted by Leslie, $60{,}000 \times (\text{Log. } 30 - \text{Log. } 17{\cdot}133,) = 14597$, exhibiting a difference of 38 feet, or about $2\frac{3}{4}$ feet in every thousand.

It thus appears that every one must discover that co-efficient for himself which is most suited to his barometer and thermometer, as well as to the method of calculation he adopts for measurements by the barometer, otherwise the heights ascertained by the barometer and the boiling points, cannot be expected to correspond. The following is the simple practical rule:—

"*Observe the boiling points under two as widely different barometric pressures as possible, and calculate the approximate* difference of height by *the method usually adopted; the difference* between the boiling points

divided by the difference of the approximate height, gives the co-efficient for after use adapted to that thermometer and barometer."

It is of no consequence whether the Centigrade or Fahrenheit's thermometer be used, provided the same scale be adhered to.

If in tropical countries the barometer usually stands at thirty inches at the level of the sea, it becomes important to ascertain what is the boiling point of the thermometer used, at that pressure; and from what I have already said, we obtain this simple rule:—

"*Observe the boiling points under two as widely different barometric pressures as possible: divide the difference of these boiling points by the difference of the logarithms of the barometer in inches, and call this* m; *this multiplied by the logarithm of one of the indications of the barometer, and the product subtracted from the elevation of the corresponding boiling points above freezing, will give a number* n: *then* m *Log.* 30 + n = 1·4771213 × m + n *gives the elevation of the boiling point above freezing at the pressure of thirty inches;* m *and* n *being multiplied by* 100, *and divided by this elevation, will adapt the formula to a thermometer graduated at* 100° *Cent. under the pressure of thirty inches, and temperature* 0°."

It is very desirable that thermometers, but particularly the boiling point instruments, be all graduated to indicate 212 Fahr. or 100° Cent. at thirty inches precisely, and when the external atmosphere is at freezing;* and each instrument maker can easily accomplish this by keeping a standard one whereby to graduate those he sells. It is probable that in this country most are now so graduated, and in that case it is necessary to ascertain with some accuracy the boiling point for any particular pressure, p.

Mr. Forbes, in his Memoir, pp. 412, 414, gives eight observations on

* In order to *compare* any other thermometer with this standard one, let a be the boiling point on the latter, (whether adapted to Fahrenheit or the Centigrade scale,) and A the boiling point of the other thermometer at the *same barometric pressure and temperature of the atmosphere:* and let $c°$ be the point where the two thermometers exactly coincide, whether that point be freezing, or above or below it: then let b be any observed state of the thermometer, this will accord with $c + \frac{a-c}{A-c}(b-c)$, or $\frac{a\,b-(a+b-A)\,c}{A-c}$ on the standard one; on the Centigrade scale, if $c = o$, $a = 100$; b becomes $\frac{100\,b}{A}$. Sometimes thermometers are made to coincide at 32° Fahr., sometimes at *Zero* Fahr.: but in order to make the degrees shown by the one correspond to those on the other, it is necessary to discover this point of coincidence, before we compare the boiling points. Usually, 32° is preferred for Fahrenheit's, in which case, the above becomes $32 + \frac{(180)\,(b-32°)}{A-32}$ or $\frac{180\,b + 32\,A - 6784}{A-32}$. The common way is to call this $b + 212 - A$, which is too small by the quantity $\frac{(A-b)\,(A-212)}{A-32}$: and if A — 212 amounts to $1\frac{1}{2}$ or 2°, and if $A - b$, be 20° or 25°, as in Saussure's observations, the error may *amount to 2 or 3 tenths* of a degree.

the boiling points by Fahrenheit's thermometer, along with the simultaneous heights of the barometer: these, arranged according to their values, are:—

1,...	Bar. pres.	30	.....	Boil. point,	212°·	.....b = 100°·344
2,...	—	28·489.....	—		210°·12.....	— 99°·3
3,...	—	25·143.....	—		204°·20.....	— 96°·011
4,...	—	23·893.....	—		201°·58.....	— 94°·556
5,...	—	23·353.....	—		200°·6	— 94°·
6,...	—	23·154.....	—		200°·10.....	— 93°·733
7,...	—	22·674.....	—		199°·08.....	— 93°·167
8,...	—	20·77	—		195°·15.....	— 90°·983

Mr. Forbes also mentions that his thermometer actually indicated 212°·62 under the pressure of 30 inches, and on deriving a formula from the 2d and 7th of the above observations, it would appear that each *had been* reduced to 212° at that pressure, by subtracting 0°·62 from the observed boiling points: the last column then indicates the boiling points after 0°·62 has been added to each of the recorded ones, and converted into centesimal degrees. In order to obtain the co-efficient m and constant n, by an average of these, we may divide the difference of the sum of the first four boiling points and that of the last four, by the difference of the sum of the logarithms of the first four pressures, and that of the last four: this gives the formula 60·189 Log. p + 11·452 = b, the thermometer showing 100°·344 at a pressure of 30 inches, or (when multiplied by $\frac{100}{100\cdot344}$,) 59·973 Log. p + 11·41 = b for a thermometer showing 100° under the same pressure. This may be said, in round numbers, to be equivalent to 60 Log. p + 11·373 = b.*

I have already deduced from Saussure's observations two formulas: the one from Leslie's data, is 61·3056 Log. p + 10·594: the other, from Mr. Forbes' account of them, is 61·0935 Log. p + 10·862. The co-efficient and constant of these being reduced so as to be adapted to a thermometer graduated to 100° at a pressure of 30 inches, are respectively 60·61 Log. p + 10°·47, and 60·43 Log. p + 10°·76. On the whole, I prefer 60·6 Log. p + 10·486 = b for the Centigrade scale, or 109 Log. p + 18·994 for Fahrenheit's, reckoned from the freezing point, or 109 Log. p + 50·994 for the precise boiling point. Dr. Horsley's formula gives 61·18 Log. p + 9·63, or in round numbers, 61·2 Log. p + 9°·6, for the Centigrade scale, and 110·1223 Log. p + 17°·32 for Fahrenheit's above 32°, or

* It may be remarked that, as by the formula just obtained from Professor Forbes' observations, 60 times the difference of the logarithms of the barometer gives the difference of the boiling points, and since 60,000 times the same gives the approximate height, therefore, 1000 times the difference of the boiling point gives the approximate height; the same rule as given by Leslie—with this distinction, that the atmosphere *is supposed* to be at the temperature of freezing, by Forbes; *by Leslie, at 5½° Cent. above it*, and that is of great importance.

110·1223 Log. p + 49·32 for the actual temperature: but it appears he obtained this from Saussure's thermometer by *equal* corrections to all the boiling points observed, which I do not consider strictly correct.

For such a thermometer, and by means of the formula I have suggested, we may discover the boiling point corresponding to any other pressure, say 20 inches, and thence derive the co-efficient by which the difference of the boiling points require to be multiplied, so as to give the approximate height. At the mean temperature of freezing, this is 990 for the Centigrade thermometer, by using the logarithmic tables, or 992½ nearly, if it be thought necessary to employ Galbraith's: the one gives 1000 for the constant multiplier, when the mean temperature of the air is 2½° Cent.; the other, when the mean temperature is 1°·8; and adapting these to Fahrenheit's thermometer, the multiplier becomes 550, at the mean temperature of 32°, using the logarithmic tables, or at the temperature of about 32·6, by using Galbraith's tables. Such give the approximate heights almost the same as by Professor Forbes' method, who takes the co-efficient 549½ also at a mean temperature of 32°, which corresponds to 550, at a mean temperature of 32°·5, and to a multiplier of 1000, when the centesimal scale is adopted, the mean temperature then being 2·8, or 2⅘° nearly, as formerly noticed by me.

The difference between these may, if thought necessary, be corrected whilst correcting the approximate height, by considering the same constant multiplier as having been fixed to correspond to different mean temperatures: twice the mean temperature adopted having to be deducted from the sum of the detached thermometers.

In the elevated mountains of India and South America, it is difficult to have a simultaneous series of observations carried on at the level of the sea, and hence, I believe, the barometer is generally assumed to stand there steadily at 30 inches: and if the thermometer be adjusted to boil at 212° Fahr., or 100° Cent. under that pressure, the difficulty of finding the approximate height by the boiling point is removed: but we have no precise method of correcting the approximate height for the variable temperature of the external air, without actual observation. At the same time, it appears to me that we may approximate to this also.

It has been long ago observed by Playfair, Leslie, and other elementary writers, (I know not who discovered that the decrease was uniform, it was confirmed by Lagrange in 1772,) that for heights in this country the mean temperature decreases 1° Fahr. for about every 90 yards, or 270 feet of ascent: and although the difference of temperature of the atmosphere between two places is by no means constant, still, in favourable circumstances, I have found that 1° Cent. for every 500 feet of ascent, is not very far from the truth. This law seems also to be applicable to Switzerland; but if I may draw conclusions from some isolated observations made by Humboldt in South America, it would appear that in the tropics it requires about 700 and sometimes 1000 feet to correspond to *1° of the same scale.* More observations require to be made on this

subject, as it may assist materially in making the correction on the heights derived from the boiling point, particularly in such situations as Dr. T. Thomson is now exploring.

Let us assume that the boiling point, under a pressure of 30 inches, is 100° Cent., then if b° Cent. be the temperature of boiling water on a mountain, and B that below, 1000 (B — b) will represent the approximate height at the temperature of 2½° by my formula; or according to Leslie, at 5° or 5½°. If in the tropics it require 700 feet of ascent to show a decrease of one centesimal degree in the temperature of the air, then $\frac{10}{7}$ (B — b) will will represent the *difference* between the observed temperatures of the air (t) at the upper and lower stations, (whether the latter be or be not at the level of the sea,) which last will be $t + \frac{10}{7}$ (B — b), and twice the sum of the temperatures is $\frac{28\,t + 20\,(B - b)}{7}$. But if we assume the constant multiplier to be 1000 for the temperature of 5°, we must deduct $4 \times 5 = 20$ from the above, leaving $\frac{28\,t + 20\,(B - b) - 140}{7}$: the correction to the approximate height is then (B — b) × $\left(\frac{28\,t + 20\,(B - b) - 140}{7}\right)$, which, added to that height, gives the

true height = H = $1000\,(B - b) + (B - b)\left(\frac{28\,t + 20\,(B - b) - 140}{7}\right)$

$= 2\,(B - b)\left(490 + 2\,t + \frac{10\,(B - b)}{7}\right)$.....R

But if it be found that 500 feet accords as well with the results for each Centigrade degree, in the tropics as in Europe, the above formula becomes—

$2\,(B - b)\,(490 + 2\,t + 2\,(B - b)$, or $4\,(B - b)\,(245 + t + (B - b))$...S

I have mentioned already my reasons for thinking that 5° is too high, and that a mean temperature of 2½ is better suited to the constant 1000; according to this view, the above two formulas become—

$2\,(B - b)\left(495 + 2\,t + \frac{10\,(B - b)}{7}\right)$.....T

$2\,(B - b)\,(495 + 2\,t + 2\,(B - b))$.....U

In reducing these last to Fahrenheit's scale, I shall take the constant 550, which answers to a mean temperature of 32°, and as $\frac{550}{275} = 2$, I shall assume that the temperature of the air decreases 1° Fahr. for every 275 feet of ascent. The corrected height then becomes

$H = (B - b)\,(510{\cdot}889 + 1{\cdot}222\,(t + (B - b))$.....V.

or in round numbers $H = (B - b)\,(511 + 1{\cdot}2\,(t + (B - b))$.....W.

But if in the tropics it requires an ascent of 700 feet to give a decrease of 1° Cent., or 390 feet for 1° Fahr., and we have to employ formula T, it, when adapted *to Fahrenheit's* thermometer, becomes

$$H = (B - b)\left(510{\cdot}4933 + \frac{100\ t + 71{\cdot}42\ (B - b)}{81}\right) =$$

$$(B - b)\ (510{\cdot}4933 + \frac{100}{81}\left(t + \frac{10\ (B - b)}{14}\right)) \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots X.$$

and in order to compare this with formula V, it may be exhibited thus:

$$H = (B - b)\ (510{\cdot}4933 + \frac{100}{81}\ (t + (B - b) \ldots\ldots\ldots\ldots\ldots\ldots Y.$$

which is obtained directly from the formula U. In both cases we may take $510\frac{1}{2}$ in place of 510·4933. By examining these two last formulas, it is obvious that if in place of 500 or 700 feet of ascent for each centesimal degree, it were $100 \times d$, then the portion to be added to t will be $\frac{10\ (B - b)}{2\ d}$, so that any one may alter the formula to suit his own experience. For although I have, for convenience, used 275 feet of ascent for each degree of Fahrenheit, or 500 for each centesimal degree, (which corresponds to about 290 for 1° Fahr.) being two degrees for each degree of difference in the boiling points, or 700 feet for each centesimal degree in the tropics, I have already said that the average number of feet, or the relation of the difference of temperature to that of the boiling points, must be ascertained by observations on the spot. Besides, the temperature of the atmosphere at different places is influenced by so many accidental circumstances, that this mode of making the correction must be regarded merely as an approximation, when actual observation at the lower station cannot be obtained, and as preferable to making no corrections at all on the approximate height.

If our object is to calculate the height above the level of the sea in the tropics, upon the understanding that water boils there at 212° Fahr., or 100° Cent., where the weather is steady, and the barometer stands at 30 inches; then it will give a result not far from the truth, if we ascertain what is the actual boiling point of the instrument at that pressure, either by observation or calculation in the way already indicated, and substitute this for B in the formula employed. Such cannot be strictly correct, because the constants have been adapted to a thermometer which really indicated 212° under the above pressure; but it must be obvious to any one, that until the thermometer has been compared with the barometer, and its boiling point at 30 inches of pressure ascertained, it is in vain to look for even an approximation in this way, to the height of the station.

In such a case, the preferable mode, perhaps, is to discover the co-efficients suited to the instrument. From what has been said, it is obvious that if the decrement of atmospheric temperature be uniform, while the difference of elevation is so, we may represent the *corrected* height by $d\ (x + yt + zd) = h$, when d is the difference between 212° (if on Fahrenheit's scale,) and the observed boiling point, and t the observed temperature of the air;—at some other elevation, as little above the level of the sea as convenient, let this be $D\ (x + y\ T + zD) = H$; and at *some intermediate height*, $\delta\ (x + y\ \tau + z\delta) = n$. Now if d, t, and h, in

the first, D, T, H, in the second, and δ, τ, η, in the third, be accurately examined, we may (by simple equations) find $z =$

$$\frac{1}{d\,\delta\,D}\left(\frac{\eta\,d\,D\,(T-t)-\delta\,(d\,H\,(\tau-t)+D\,h\,(T-\tau))}{(d-D)\,(\tau-t)-(d-\delta)\,(T-t)}\right) =$$

$$\frac{1}{d\,\delta\,D}\left(\frac{\delta\,(\tau-t)\,(h\,D-H\,d)-D\,(T-t)\,(h\,\delta-\eta\,d)}{(d-D)\,(\tau-t)-(d-\delta)\,(T-t)}\right), \text{ and}$$

$$y = \frac{z\,d\,D-(h\,D-H\,d)}{d\,D\,(T-t)} \text{ and } x = \frac{h-d\,(y\,t+2\,d)}{d}$$

which values being substituted in the equation $d\,(x + y\,t + 2\,d)$ gives us a formula for that thermometer, and the decrement of atmospheric temperature for the climate, whatever it be, so that we may calculate by it other elevations.

I shall now illustrate some of the formulas given above by five examples

Ex. 1. Dr. T. Thomson writes that at one place, which he considered to be at an elevation of about 18,500 feet, he observed the boiling point to be 180°·3 Fahr., which corresponds to 82°·39 Cent. Now I am ignorant under what pressure his instrument is graduated, and also of the temperature of the external air. The former probably showed 100° when the barometer stood at 30 inches, and the latter, from the season of the year and collateral remarks made by Dr. T. Thomson, may be assumed to be freezing, or 0°. In formula S, then, $t = o$, $B - b = 17{\cdot}61$, whence the height $= 35{\cdot}22\,(490 + 35{\cdot}22) = 18{,}498$ feet, differing only 2 feet from the height suggested by Dr. Thomson. But if we take the formula U, which I prefer, the height is 18,674, or 172 feet more than supposed by Dr. Thomson.

Ex. 2. In the extract from Dr. T. Thomson's letter, at the commencement of these remarks, he states that his thermometer showed the boiling point to be 203°·6, while the temperature of the atmosphere was 80°. These on the Centigrade scale (which I prefer for its simplicity in calculation) are 95°·333, and 26°·667 respectively: hence $t =$ 26°·677, and $B - b =$ (supposing B = 100°, as in the first example) $= 4{\cdot}667$. The height, by formula S, is thus 5156, or by formula U, 5203 feet. By using Fahrenheit's thermometer, the formula X gives the height = 5147, and W, 5198. Unfortunately, Dr. Thomson does not state what he considers the height of this station to be.

Ex. 3. Humboldt observed the barometer on Chimborazo to be at 0·377275 metres, or 14·853 English inches; and the detached thermometer to indicate — 1°·6 Cent. The boiling point of a thermometer adjusted to 100° Cent. at a pressure of 30 inches, would thus, by my formula (60·6 Log. p + 10·486 = b) be on the mountain 81°·5. In formula U, then, $t =$ — 1°·6, $B - b = 18{\cdot}5$, whence the height = 37 (495 —3·2 + 37) = 19,566. The true height, after making every correction for the hygrometer, latitude, &c. &c., is 19,441: the error, 125 feet, arising principally from it requiring 700 feet of ascent for each *degree, whereas I have only* allowed 500.

Ex. 4. Humboldt observed the barometer on Quindiu to stand at 0.509818 metres, or 20·072 English inches, and the detached thermometer to be at 18°·75 Cent. The boiling point of a thermometer, by my formula, would thus be 89°·43. In formula U, $t = 18°{\cdot}75$, $B - b = 10{\cdot}57$; whence the height is 11,703 feet; the height obtained, by the most refined barometrical methods, is about 11,500, so that there is here an error of about 200 feet. But this example is an unfair one, for there was only a difference of 6°·55 Cent. observed between the temperature of the atmosphere on the mountain, and that at the level of the sea, whereas by my general formula, it is supposed to be 21°. In other words, while Fahrenheit's thermometer stood at 77°·54 at the level of the sea, it was as high as 65°·75 at the great elevation of upwards of 11,000 feet: such must have arisen from accidental or local causes, and it has always appeared to me that *in such cases*, some new correction must be applied before we can make use of that element in the calculation of heights by the barometer.

Ex. 5. The last example I shall give, is taken from Professor Forbes' Observations, (p. 415 of his paper on this subject:) he states, that the thermometer having been corrected to show the boiling point at 212° Fahr., or 100° Cent., at a pressure of 30 inches, indicated the boiling point to be 191°·93 or 88°·85 Cent. on the Col d'Erin in the Vallais, the temperature of the external air being 34° Fahr., or 1°·11 Cent. At Geneva, the boiling point was not observed, but the barometer stood at 28·73 inches. Now 28·73 corresponds, by my formula, to the boiling point of 98·86; whence in formula U, $t = 1{\cdot}11$, $B - b = 10{\cdot}01$, and the height above Geneva is $20{\cdot}02\ (495 + 2{\cdot}22 + 20{\cdot}02) = 10{,}355$. According to Professor Forbes, the height was 10,377, partly by the ordinary barometric calculation, and partly by his own constant multiplier: this makes the height of the Col d'Erin, above the level of the sea, according to him, 11,720 feet; by the formula U, 11,698, or almost 11,700 feet. If we suppose that the boiling point was 100° at the level of the sea, then $t = 1°{\cdot}11$ Cent., $B - b = 11{\cdot}15$, and the altitude is 11,585; on the same supposition, Mr. Forbes makes it 11,586. But water by that thermometer must actually have boiled at the level of the sea at that time, at 212°·225, or 100°$\frac{1}{8}$ Cent.

In giving these formulas, I do not intend them for the man of science, for I suppose there are few such who have not had recourse to similar methods of correcting the approximate heights given by their barometer, in default of actual simultaneous observations on the temperature of the atmosphere at both stations; but because they may prove useful to those who work only by the formulas of others. Whenever a barometer is at hand, it is a much more trustworthy instrument than the best thermometer adapted to take the boiling points, for as justly observed upon the latter, by Professor Forbes, "in no circumstances, even the most favourable, is the observation true to less than $\frac{1}{20}$ of a degree." But there are few *who have travelled in* mountainous districts who cannot tell of barometers

broken, or mercury escaping, and air getting into the tube: so that it is desirable to render the thermometer as useful an auxiliary as possible. Fortunately, in a botanical point of view, which most concerns me, and which must be my excuse for bringing forward this notice of a subject apparently so unconnected with my own profession, great nicety is not required in ascertaining the elevations.

Dr. R. D. Thomson gave a notice of the sequel of Dr. Thomas Thomson's expedition into Thibet, which detailed his successful attempt to reach the Karokoram pass into Central Asia, his discovery of a range of mountains 24,000 feet high, and his approximation to the source of the Shayok or northern branch of the Indus. It is believed, that he has been the first European who has succeeded in attaining such a high latitude in this part of India. The details have been published in Hooker's Journal of Botany, and in the Geographical Society's Journal.

On the motion of Mr. Crum, it was agreed that £70 shall be appropriated to the purchase of Books and Periodicals.

3d January, 1849.—*The* PRESIDENT *in the Chair.*

THE following gentlemen were admitted members:—Messrs. James Beith, Dugald Bannatyne, Michael Connal, James A. Campbell, John Elder, William Ferguson, Robert M'Laren, George Paterson, Neil Robson.

Mr. Gourlie mentioned that a number of bones, belonging to the extinct bird of the Island of Rodrigues, called the solitaire, and the property of the Andersonian Museum, had been laid on the table. Figures of all these bones, which are of great rarity, have been given in the work—"The Dodo and its Kindred," by Mr. Strickland and Dr. Melville, to whom the Trustees kindly lent them, for the purpose of illustrating their work, a copy of which had been presented to the Museum by the authors.

III.—*On the Structure of Staffa and the Giant's Causeway.*—By JAS. BRYCE, Jun., Esq., M.A., F.G.S.

THE author stated and illustrated some new views regarding the structure of Staffa and the Giant's Causeway. He first explained the general structure of the basaltic district of north-eastern Ireland. It occupies an area of more than 1000 square miles, and consists of a great substratum of new red sandstone, supporting three distinct fossiliferous beds, lias, greensand, and chalk, which are overlaid by a thick covering of trap rock of igneous origin, similar to that which forms the Giant's Causeway. *These three fossiliferous rocks* are absent in the great natural section

formed by the mural precipices of the Causeway coast. It was shown that this has been occasioned by a great fault, whereby the whole promontory of Bengore, including the Causeway itself, and a tract of country five miles long by one to one-and-a-half wide, has been thrown down about 400 feet, and thus the upper beds of the superincumbent trap are brought to the same level as the chalk. The same fault was shown to embrace also a portion of the island of Rathlin.

The various beds of the Causeway cliffs were then described as rising in succession from the sea level in Port-Moon, and attaining their highest level in Pleaskin, whence they again descend in a long curve towards the north of the Causeway, which is formed by the intersection of one of the beds with the sea line. The beds rise again towards the west and run out in succession, owing to the gradual lowering of the cliffs.

The beds forming the façades of Staffa were next described, and were shown to have a remarkable resemblance, both in mineral character, in succession, and in thickness, to those of the Causeway.

Some theoretical views were then stated respecting the origin of these remarkable formations, and the former existence of separate volcanic *foci* at the Giant's Causeway and Staffa, and on the probable connection of these with movements of the surface still taking place along the great Caledonian valley, and other lines parallel to the axis of the Grampian chain.

IV.—*Note on the altered Dolomites of the Island of Bute.* By JAMES BRYCE, JUN., M.A., F.G.S.

IN the closing paragraphs of a paper on the geology of Bute, read before the Society last session, and since published in their Journal, I described certain changes which have been produced upon the Kilchattan limestone by contact with igneous rocks. These changes, however, were then but approximately determined,—the limestone having been subjected by Mr. John Macadam, at my request, merely to a *qualitative* analysis,—sufficient to indicate the character of the change, but of too general a nature to ascertain the exact amount of change, as to afford definite terms of comparison with the analyses of other limestones. Careful *quantitative* analyses of a series of specimens have now been obtained through the kindness of Dr. Robert D. Thomson;—they have been made under his care in the laboratory of the University; and his name is a sufficient guarantee for their accuracy. It is hoped that the publication of these may lead to the formation of clearer views on an obscure question in theoretic geology.

The analyses kindly furnished to me by Dr. Robert D. Thomson, are as follows:—

Specimen No. 1, is the saccharine marble from the contact with the dike at Kilchattan, in the highest state of alteration. It is the same as *No. 1 in Mr. Macadam's* statement in my former paper.

No. 2, is the hard crystalline marble, having the crystals in distinct flakes—more remote from the dike and less altered than No. 1.

No. 3, is the unaltered limestone from the middle of the quarry, remote from the dike—an average specimen. It corresponds to No. 2, in Mr. Macadam's report.

No. 4, is the altered limestone from contact with the overlying trap at Ascog mill; it is an impure, dark coloured rock, of earthy aspect, and very like the trap which rests upon it.

The matter termed silica and alumina, is what was insoluble in muriatic acid.

	No. 1.	I.	II.
	Analyses by Mr. J. H. Turnbull.	Analyses by Mr. Henry S. Thomson.	
Specific gravity, 2·710			
Silica, Alumina,	6·91	5·16	5·70
Protoxide of iron,	1·68	1·50	1·28
Carbonate of lime,	90·65		91·08
Carbonate of magnesia,	1·00		1·17
	100·24		99·23

	No. 2.	I.	II.
Specific gravity, 2·570			
Silica, Alumina,	1·94	0·28	0·28
Protoxide of iron,	0·52		0·56
Carbonate of lime,	96·48	98·76	96·58
Carbonate of magnesia,	1·23		2·24
	100·17		99·66

	No. 3.	I.	II.
		Specific gravity, 2·679	
Silica, Alumina,	9·70		9·08
Protoxide of iron,	1·12		1·12
Carbonate of lime,	67·42	72·12	67·00
Carbonate of magnesia,	17·31		18·06
Water, coaly matter, and carbonic acid,	4·45		4·74
	100·00		100·00

	No. 4.
Silica, Alumina,	64·21
Protoxide of iron,	6·42

Carbonate of lime,	24·00	21·20
Carbonate of magnesia,	4·62	2·85
Water & carbonic acid,	1·75	4·89
	100·00	100·00

These analyses are confirmatory of the views stated in my former paper, and bear out the principal points in Mr. Macadam's report; they seem clearly to establish the new and remarkable fact, that by the igneous action in these localities in Bute, magnesia has been driven off from the limestone. The unaltered rock is a dolomite, containing nearly 70 per cent. of carbonate of lime, and nearly 20 per cent. of carbonate of magnesia; while the altered rock contains no more than 1 or 1½ per cent. of the latter ingredient. To what cause are we to assign the changes that have taken place? Has the magnesia been sublimated by the action of the trap dike when fluid under the influence of heat? or has it been withdrawn by the solvent power of free carbonic acid, after the consolidation of this matter from a state of igneous fusion? On the nature of these and the other chemical changes that have been induced, and on the precise character of the metamorphic action, it is unnecessary that I should make any additional remarks, the subject having been, perhaps, as fully treated of in my former paper, as my own knowledge of theoretical chemistry, and our present limited acquaintance with facts, will permit. The subject is one of great interest, both to the geologist and chemist, as the facts are directly opposed to the received views, (*Daubeny on Volcanoes, 2d Edit.* 1849,) and as no instance of similar changes on dolomitic rocks has, so far as I am aware, ever been put on record. It is hoped that the publication of these analyses may lead to a similar examination of dolomitic rocks placed under like conditions, and that thus, by an extended inquiry, materials may be collected for establishing the true theory of the chemical changes which these rocks have undergone.

17th January.—The PRESIDENT *in the Chair.*

THE following gentlemen were admitted, viz.:—Messrs. James Stevens, William Johnson, and David Y. Stewart.

On the motion of Mr. Cockey, seconded by Mr. Bell, it was agreed that the entire Catalogue of the Library, now nearly exhausted, should be printed, instead of a supplement only, as was formerly proposed. For this purpose £7 was voted for the first time.

Mr. Glassford explained and exhibited the new method of applying electricity to the purpose of illumination. Mr. Glassford, in conjunction with Mr. R. Finlay, philosophical instrument maker, had constructed a *very extensive and* powerful galvanic battery, upon the Maynooth principle,

for the express purpose of showing the electric light, and for testing its applicability to the purposes of ordinary life. Mr. Glassford described the different schemes which had been tried by Mr. Staite, the patentee of the electric light, to render this brilliant source of light available for the ordinary purposes of illumination. It is well known that a most intense and beautiful light is emitted when pieces of wood charcoal, connected to the poles of a large battery, are brought in contact and then slightly withdrawn from each other. This, until within the last few years, was the only method known and practised for showing the electric light. From the great combustibility of the charcoal points, they were quickly consumed, and of course the light was extinguished. The luminous power of the galvanic fluid was known as early as 1810, and many experiments were made with the large battery of the Royal Institution, London, and the illuminating phenomena produced were of the most brilliant and dazzling description.

That such a splendid light might be made available in ordinary life, doubtless suggested itself to many, and numerous trials were probably made with this view. But it appears that M. Achereau of Paris was the first who really succeeded in keeping up a constant light, and in actually applying it to ordinary illuminating purposes. This was done in 1843, at the Place de la Concorde in Paris, but the trial, although eminently successful in demonstrating its practicability, had no further result, and for a long time it seems to have been lost sight of. Mr. Staite has again brought the subject before the world, and has patented an ingenious apparatus, whereby the light can be regulated by the same means that the light is produced. He has also introduced more suitable materials than wood-charcoal as electrodes, or light-conducting points; and has apparently almost overcome the mechanical difficulties in practically applying electricity to the purpose of illumination. The machinery employed for producing the various upward, downward, and horizontal motions, is an adaptation of clock machinery, and the motive power is obtained by magnetising a bar of soft iron with a current of the electricity while on its way to the points of illumination. Instantly the current of electricity is established, the bar of iron—round which, in a circle of small wire, the current flows—becomes magnetic, and attracts a small piece of iron in its immediate vicinity, which is attached to one of the small clock wheels. The instant the smaller bar of iron moves, the whole machinery is set in motion, and the electrodes, or tubes holding the illuminating points, are drawn asunder. The electric fluid now traverses a stratum of air, and sheds its intense and dazzling light, and in proportion to the power of the battery, so is the distance of these electrodes from each other, and so is the volume of light great or small. If the points are drawn too far apart, the current will suddenly cease, and the light disappear. On this the small bar of iron, before under the influence of magnetism, loses its power and falls into its former position; in consequence of which the motion of *the machinery is reversed*, and now the illuminating points are rapidly

brought together. When again brought in contact, the current is established, the motion reversed, the points are withdrawn from each other, and again the light shines out. The light so produced is of the most intense brilliancy, and is overpoweringly dazzling. It is beautifully pure and white, and exactly resembles in power and properties the solar light. Mr. Glassford employed a variety of electrodes, such as of common coke, plumbago, and mixtures of these, and explained the methods of preparation, and their comparative properties for conducting the fluid. The battery employed on this occasion, consisted of sixty-three large-sized Maynooth cells. Mr. G. displayed its extraordinary heating power by the fusion of pieces of steel, iron, and copper. The chemical and magnetising powers of this battery he described as being also very astonishing. There are several disadvantages in the use of the Maynooth battery, which Mr. G. thinks will be overcome, and which will render this the most effective, cheap, and useful electrical instrument known. Much of the success in the application to ordinary illuminating purposes of the electric light, depends on the economy with which the electric power can be got up and maintained; all the other obstacles are trifling compared with this.

31*st January*, 1849.—*The* PRESIDENT *in the Chair.*

THE following members were elected, viz.:—Dr. James Jeffray, Mr. John Jeffray, Mr. Andrew Stein, Mr. Andrew Laughlen, Mr. Robert Anderson.

The Librarian announced the presentation of Transactions of the Royal Scottish Society of Arts, Parts II. and III. of the third volume; and the Third Report of the Dublin University Museum.

The second vote was taken on the proposal to grant £7 for the reprinting of the Library Catalogue, which was finally agreed to.

Professor Gordon made some remarks on the proposal to substitute Locomotive Carriages for Locomotive Engines and Passengers' Trains on Railways. He commenced by describing Mr. Samuel's Express Engine, on which he (the Professor) last year travelled from London to Cambridge at the rate of 32 to 44 miles an hour, and which, with an engine weighing 18 cwt., together with boiler, wheels, framing, seats for 7 or 8 passengers, &c., weighed altogether between 22 and 23 cwt. It was this engine that suggested to Mr. Adams of Fairfield, the practicability of the plan for which he took out a patent last year; for observing that the express engine could do so much more work with a 3 inch cylinder, a 6 inch stroke, and 3 feet wheel, he supposed that an engine not very different in its proportions might be attached to a carriage; and hence he projected the combination of locomotive and carriage. The locomotive carriage constructed by him, and which Mr. Gordon next described, weighed, the locomotive 6 tons, and the carriage 7 or 8 tons, and carrying 50 passengers—*the total weight* was about 18 tons. This he contrasted with the

locomotive on the Great Western, named the "Iron Duke," weighing 36 tons, and the tender 14 tons, making 50 tons, with water for a run of about 50 miles. Professor Gordon then briefly sketched the history of the present railway system, which may be considered as having originated with the Stockton and Darlington Company in 1825, when the locomotive engine was yet in its infancy. The system was further developed in 1829 by the formation of the Liverpool and Manchester railway. The Professor called especial attention to the circumstance, that up till 1830, the success of the system was held to depend chiefly on goods traffic. But it soon became apparent that the traffic in goods, for which the Liverpool railway was principally constructed, was subordinate to the passenger traffic, the development of which Mr. Stephenson, Mr. Locke, and other eminent engineers, soon discovered to be—what is now universally admitted —the real source of the prosperity of the railway system. Mr. Gordon proceeded to discuss the questions of the present average weight of locomotive engines and tender—the amount of adhesion—the power and efficiency of the engine. Mr. Stephenson's engine, the "Rocket," constructed in 1829, according to the condition on which the Directors of the Liverpool and Manchester railway offered a premium, weighed 4 tons 5 cwt.; its tender, with water and coke, 3 tons 4 cwt.; and its power was equal to drawing two carriages, weighing, with load, 9½ tons, at the rate of 15 miles per hour. The locomotive was afterwards gradually increased in weight to 6, 8, 10, and, in 1834, to 12 tons; and it has subsequently been increased to a weight varying from 18 to 25 tons on narrow gauge lines, and 24 to 35 on broad gauge lines, including water and coke; while the tender has been increased, on narrow gauge lines to 29 tons, and on broad gauge lines to 42 tons. This increase of weight, it was shown, was rendered necessary by the demand for an increase of power, which could not be obtained without a corresponding increase of adhesion. The adhesion, being the friction between the surface of the rail and the periphery of the driving-wheel, bears a certain proportion to the insistent weight on the driving-wheels, varying with the state of smoothness of the rail and wheels, according to Mr. Stephenson, and from one-sixth to one-thirteenth of the insistent weight, the amount of adhesion to be relied on depending on the weight of the train and the gradients on the line. As to the efficiency of the engine, it was shown, that all engines, as engines, might be made equally efficient. The question was not here as to their absolute efficiency, but as to the part of the effective power of the locomotive engine absorbed by the work done, *i.e.* the transport of a certain weight of passengers. It appeared from the data examined by the Professor, that for the ordinary passenger traffic on first class railways, from 55 to 60 per cent. of all the *effective* power expended is required to move the locomotive itself; and in railways of small traffic, the modern locomotive absorbs from 60 to 75 per cent. of the power developed in the cylinders, or at the periphery of the driving-wheels.

14th February, 1849.—MR. CRUM *in the Chair.*

MR. JAMES PATERSON, Assay Master, was elected a Member.

A copy of Report on Davies's Rotary Engine was presented to the library by Mr. William Johnson.

Professor William Thomson gave "An Account of Mr. Faraday's recent Discoveries relative to the Magnetic Condition of all Matter."

Mr. John Bryce exhibited and explained a working model of a Hydrostatic Pressure Regulator.

28th February, 1849.—*The* PRESIDENT *in the Chair.*

ON the motion of Mr. Crum, seconded by Dr. A. K. Young, the Society agreed to record its regret at the loss it has sustained by the death of George Watson, Esq. Surgeon, one of the original members, and a member of the Council.

Professor Gordon continued his remarks on the proposal to substitute Locomotive Carriages for Locomotive Engines and Passengers' Trains on Railways.

Mr. Andrew Shanks exhibited a Model of Dunn's Patent Improved Mode of Removing Railway Carriages from one Line to another.

14th March, 1849.—*The* PRESIDENT *in the Chair.*

DR. R. D. THOMSON made a statement of the Comparative Fall of Rain at Glasgow and various parts of Scotland and England, and communicated the following tables:—

TABLE I.

	BOTANIC GARDEN, GLASGOW.	IBROXHOLM.			Mean at Ibrox-holm.	GREEN-OCK.
	1847.	1848.	1846.	1847.	1848.		1848.
January,	—	2·01	3·36	2·29	1·40	2·35	3·2
February,	—	6·73	2·90	1·79	7·64	4·11	11·8
March,	—	2·45	2·89	0·56	2·30	1·91	4·4
April,	2·44	1·27	1·43	2·56	1·29	1·76	1·35
May,	3·38	0·90	1·62	3·66	0·66	1·98	2·2
June,	2·25	3·97	4·18	2·52	3·38	3·36	3·8
July,	1·63	2·75	4·28	1·40	2·73	2·80	5·3
August,	1·41	3·45	4·56	1·39	3·10	3·01	3·8
September,	2·51	1·59	2·40	2·33	1·28	2·00	2·2
October,	4·09	3·00	4·11	5·42	2·63	4·05	4·7
November,	3·78	4·53	3·32	5·11	5·57	4·66	6·9
December,	4·00	3·33	0·86	4·30	3·88	3·01	6·3
Mean Annual Rain,		35·98	35·91	33·33	35·86	35·00	55·95

The Register at Botanic Garden kept by S. Murray, Esq.; at Ibroxholm by T. R. Gardiner, Esq.; and at Greenock, on the Gourock Road, by W. Davidson, Esq. M.D.

Mean at Greenwich for seven years, 25·8 inches, (1841—47, inclusive.)



TABLE II.

EVERTON COTTAGE, GREENOCK, 29*th January*, 1849.

The following is a statement of the rain here since 1835, with the exception of 1840; the water wasted in 1839, was also neglected, consequently the available quantity of rain cannot be ascertained. The following is taken from the books, showing the quantity in each guage. Everton Garden is 472 feet; No. 1, 600 feet; No. 2, 560 feet; No. 3, 800; and No. 4, 540 feet above the level of the sea. Mean height of the five guages = 594 feet.

1835.	Everton Garden.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	3·40	4·10	3·80	3·10	3·20
February,.	6·60	7·10	7·80	5·30	5·60
March,.....	4·85	4·70	4·70	3·60	3·70
April, ...	2·30	2·60	2·55	2·20	2·10
May, ...	6·55	7·40	7·30	6·50	6·30
June, ...	2·50	2·60	2·65	2·30	2·20
July, ...	5·00	5·25	5·20	5·10	5·30
August, ...	4·40	6·60	6·70	5·50	5·20
September,	8·30	8·80	9·00	8·00	8·50
October, ...	4·25	5·20	5·10	4·70	4·40
November,	9·70	9·50	10·30	8·70	9·10
December,	5·30	5·50	5·60	4·30	5·20
	63·15	69·35	70·70	59·30	60·30

RAIN IN 1835.
Mean of the five guages = 64·66 inches,
Of which quantity 19·68 inches was lost by evaporation, &c., and 44·98 inches available.

1836.	Everton Garden.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	9·00	8·20	8·30	7·80	8·20
February,.	3·40	4·60	4·80	4·30	4·60
March,.....	8·60	8·80	8·70	8·60	8·70
April, ...	3·70	4·10	4·00	4·00	4·10
May, ...	0·40	0·70	0·60	0·50	0·50
June, ...	3·90	4·60	4·70	4·50	4·60
July, ...	9·00	9·20	9·10	9·00	9·10
August, ...	5·00	5·20	5·10	5·00	5·10
September,	8·40	8·30	8·20	8·30	8·20
October, ...	5·10	5·00	5·00	4·90	5·00
November,	8·00	7·80	7·80	7·60	7·80
December,	9·50	8·70	8·70	8·60	8·70
	74·00	75·20	75·00	73·10	74·60

RAIN IN 1836.
Mean = 74·38 inches,
52·68 inches available, and
21·70 inches lost by evaporation, &c.

1837.	Everton Garden.	No. 1.	No. 2.	No. 3.	No. 4
January,...	4·40	4·70	4·60	4·60	4·70
February,.	7·00	7·20	7·10	6·50	7·00
March,.....	1·90	2·00	1·80	1·50	1·70
April, ...	3·20	3·40	3·70	3·20	3·40
May, ...	2·70	2·50	2·60	2·50	2·60
June, ...	3·00	3·10	3·00	2·90	3·00
July, ...	3·30	3·40	3·40	3·30	3·40
August, ...	4·45	4·50	4·50	4·50	4·60
September,	3·45	3·50	3·60	3·40	3·50
October, ...	7·40	6·90	7·00	6·70	7·00
November,	7·20	7·00	7·10	7·00	7·10
December,	7·30	7·80	7·90	7·30	7·10
	55·30	56·00	56·30	53·40	55·10

RAIN IN 1837.
Mean = 55·22 inches.
40·26 inches was available, and
14·96 inches lost by evaporation, &c.

1838.	Everton Garden.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	2·00	2·00	1·90	1·30	1·40
February,.	1·10	0·90	0·95	0·80	1·00
March,.....	6·20	6·70	6·80	6·40	6·90
April, ...	3·10	3·40	3·00	3·00	3·30
May, ...	2·00	2·10	2·20	2·00	2·10
June, ...	5·00	5·60	5·40	5·10	5·30
July, ...	6·50	6·90	7·00	6·90	6·80
August, ...	7·10	6·50	7·00	6·50	6·00
September,	5·10	5·10	5·00	5·00	5·20
October, ...	8·00	7·30	7·40	7·20	7·30
November,	7·00	6·50	6·50	6·30	6·50
December,	7·80	6·90	6·90	5·80	5·90
	60·90	59·90	60·05	56·30	57·70

RAIN IN 1838.
Mean = 58·97 inches.
38·08 inches was available, and
20·89 inches lost by evaporation, &c.

1839.	Everton Garden.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	4·90	5·40	5·50	5·40	5·20
February, .	6·50	6·10	6·80	5·70	6·80
March, ...	6·60	6·60	6·80	5·80	6·70
April, ...	2·45	2·90	2·40	2·10	2·40
May, ...	3·30	3·40	3·40	3·30	3·40
June, ...	2·30	2·50	2·20	2·10	2·20
July, ...	5·10	5·40	4·90	5·30	5·00
August, ...	5·10	5·20	5·00	4·90	5·00
September,	8·00	7·40	7·50	7·20	7·40
October, ...	6·20	6·40	6·30	6·40	6·00
November,	4·50	4·50	4·30	4·00	3·80
December,	6·20	6·40	6·40	6·20	6·30
	61·15	62·20	61·50	58·40	60·20

RAIN IN 1839.
Mean of five guages = 60·69 inches.
The quantity of water used and wasted has not been kept this year, so I cannot ascertain the quantity available and lost. Also, the rain in 1840 has been neglected.

1841.	Everton Garden.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	5·10	5·20	5·20	5·10	5·10
February, .	5·50	5·50	5·40	5·30	5·30
March, ...	5·50	5·10	5·00	5·00	5·10
April, ...	3·90	4·10	4·00	4·00	3·90
May, ...	4·00	4·10	4·10	4·00	4·00
June, ...	3·10	5·00	5·10	5·10	4·90
July, ...	3·70	3·30	3·20	3·20	3·20
August, ...	6·10	5·70	5·60	5·40	5·50
September,	7·10	6·50	6·60	6·50	6·70
October, ...	6·10	6·50	6·80	6·50	6·40
November,	4·20	4·70	5·00	4·00	4·50
December,	7·50	8·20	8·20	7·60	8·10
	61·80	63·90	64·20	61·70	62·70

RAIN IN 1841.
Mean = 62·86 inches.
45·92 inches was available, and
16·94 inches lost by evaporation, &c.

1842.	Everton Cottage.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	3·90	3·30	3·40	3·00	3·20
February, .	5·90	5·80	6·00	5·30	5·40
March, ...	8·00	8·00	8·20	7·80	8·00
April, ...	—	—	—	—	—
May, ...	4·00	4·30	4·40	4·10	5·40
June, ...	3·80	2·80	3·00	2·60	3·20
July, ...	3·00	3·70	3·50	3·60	3·70
August, ...	3·70	4·30	4·20	4·10	4·40
September,	3·40	3·10	3·20	3·00	3·10
October, ...	1·70	2·10	3·00	2·90	2·60
November,	3·80	4·20	4·40	4·10	4·00
December,	10·50	13 00	12·80	12·70	12·60
	51·70	54·60	56·10	53·20	55·60

RAIN IN 1842.
Mean of five gauges = 54·24 inches.
37·50 inches was available, and
16·74 inches lost by evaporation, &c.

1843.	Everton Garden.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	7·00	11·00	9·10	16·00	12·00
February,'.	1·70	1·60	1·30	1·20	1·30
March,.....	2·50	2·70	2·90	2·80	2·40
April,.....	5·35	5·60	6·00	5·40	5·60
May, ...	3·30	3·50	3·70	3·30	3·40
June,.....	3·80	3·00	4·80	3·20	3·60
July,.....	4·80	4·60	4·50	4·20	5·00
August, ...	4·20	3·60	5·00	4·60	5·10
September,	3·00	3·70	3·50	3·00	3·40
October, ...	6·80	7·80	8·40	8·00	8·20
November,	7·10	8·20	8·50	8·00	8·10
December,	3·25	5·10	5·20	5·10	5·10
	52·80	60·40	62·90	64·80	63·20

RAIN IN 1843.
Mean of five guages = 60·82 inches.
38·78 inches was available, and
22·04 inches lost by evaporation, &c.

1844.	Everton Cottage.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	4·40	6·10	6·20	6·00	6·10
February,..	3·40	5·70	5·70	5·60	5·80
March, ...	5·00	5·20	5·30	5·10	5·40
April,.....	3·25	4·50	4·40	4·00	4·60
May,.....	—	—	—	—	—
June,.....	6·10	6·40	6·50	6·70	6·80
July, ...	3·90	3·60	3·65	3·60	3·70
August, ...	2·45	2·30	2·50	2·50	2·40
September,	2·60	2·70	2·80	2·90	2·65
October, ...	3·65	4·30	4·20	3·60	3·60
November,	5·40	5·95	6·00	5·70	5·50
December,	0·50	0·50	0·60	0·50	0·50
	40·65	47·25	47·85	46·20	47·05

RAIN IN 1844.
Mean of five guages = 45·80 inches.
29·21 inches available, and
16·59 inches lost by evaporation, &c.

1845.	Everton Cottage.	No. 1.	No 2.	No. 3.	No. 4.
January,...	6·60	7·50	7·60	7·00	6·30
February,..	2·75	3·40	3·45	3·20	3·00
March, ...	4·40	5·40	5·40	5·00	4·50
April, ...	3·90	4·20	4·15	3·80	3·70
May, ...	1·15	1·80	1·75	1·60	1·50
June, ...	5·00	5·40	5·80	5·00	5·10
July, ...	3·40	4·20	4·40	3·80	4·00
August, ...	2·60	3·00	2·90	2·50	2·50
September,	5·00	5·80	5·90	5·50	5·90
October, ...	9·20	11·55	11·60	10·50	11·40
November,	9·00	8·00	9·70	8·60	9·60
December,	8·15	8·30	9·00	10·00	10 95
	61·15	68·55	71·65	66·50	68·45

RAIN IN 1845.
Mean of five guages = 67·26 inches.
46·74 inches available, and
20·52 inches lost by evaporation, &c.

1846.	Everton Cottage.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	7·80	7·00	7·50	7·75	7·70
February,..	4·60	5·00	5·50	4·80	4·50
March, ...	7·10	6·80	7·30	5·70	6·60
April,.....	2·20	2·40	2·40	2·30	2·10
May,.....	3·10	3·30	3·50	3·40	3·40
June,.....	5·90	6·00	6·10	5·90	5·60
July,.....	8·35	9·60	11·70	9·80	9·40
August, ...	6·70	6·60	7·80	6·20	6·40
September,	3·70	4·00	4·00	3·90	3·50
October, ..	7·30	7·50	7·60	7·00	6·80
November,	6·80	6·90	7·10	6·00	6·65
December,.	1·50	2·00	2·00	1·90	1·80
	65·05	67·10	72·50	64·65	64·45

RAIN IN 1846.
Mean of five guages = 66·75 inches.
52·09 inches available, and
14·66 inches lost by evaporation, &c.

1847.	Everton Cottage.	No. 1.	No. 2.	No. 3.	No. 4.
January, ...	3·60	3·30	3·40	3·00	3·40
February,..	3·00	3·10	3·40	2·60	2·70
March,.....	3·20	2·40	2·40	2·50	2·20
April, ...	4·10	4·20	4·50	4·30	4·10
May,.....	4·15	4·40	4·60	4·20	4·40
June,.....	2·55	2·80	3·00	2·70	2·80
July, ...	3·20	3·10	3·20	3·00	3·20
August, ...	3·50	3·90	4·30	3·60	3·70
September,	5·30	5·60	6·60	6·60	5·80
October, ...	8·10	8·50	8·60	8·10	8·20
November,	8·30	9·70	10·00	8·30	8·70
December,.	8·20	8·20	9·00	7·20	6·70
	56·20	59·20	63·00	56·10	55·90

RAIN IN 1847.
Mean of five guages = 58·08 inches.
41·28 inches was available, and
16·80 inches lost by evaporation, &c.

1848.	Everton Garden.	No. 1.	No. 2.	No. 3.	No. 4.
January,...	3·30	3·40	3·60	2·80	3·10
February,..	11·70	13·50	14·00	13·10	13·30
March, ...	4·80	5·10	5·40	4·70	5·20
April,.....	1·60	1·70	1·70	1·50	1·60
May,.....	2·90	3·10	3·50	2·60	3·00
June,.....	5·50	5·10	5·30	5·50	5·00
July,.....	6·20	6·30	6·70	4·30	4·40
August, ...	4·50	4·50	4·80	4·20	4·00
September,	2·70	2·80	3·30	3·10	2·50
October,....	5·00	5·30	5·80	5·70	5·20
November,	7·90	8·10	8·50	6·30	7·50
December,.	7·10	6·70	12·40	6·30	10·50
	63·20	65·60	75·00	59·50	65·30

RAIN IN 1848.
Mean of five guages = 65·72 inches.
48·76 inches was available, and
16·96 inches lost by evaporation, &c.

TOTAL MEANS.

1835,.....	64·66
1836,.....	74·38
1837,.....	55·22
1838,.....	58·97
1839,.....	60·69
1840,.....	62·86
1842,.....	54·24
1843,.....	60·82
1844,.....	45·80
1845,.....	67·26
1846,.....	66·75
1847,.....	58·08
1848,.....	65·72
	61·18

At a mean height of 594 feet.
Mean of the lowest guages, 59 inches.

THE LARGEST QUANTITY OF RAIN THAT HAS FALLEN IN 24 HOURS, IN EACH OF THE ABOVE MENTIONED YEARS:—

1835,.....September	22d,	.....1·60	inches.
1836,..... —	3d,	.....2·00	—
1837, 1838, 1839.*			
1841,.....September	2d,	.....1·40	—
1842,.....March	3d,	.....2·00	—
1843,.....July	17th,	.....1·55	—
1844,.....June†	12th,	.....1·30	inches.
1845,.....April	25th,	.....1·40	—
1846,.....July	5th,	.....2·40	—
1847,.....October,	6th,	.....1·50	—
1848,.....December,	3d,	.....1·50	—

* The rain for these years has been kept only monthly.

† Also same quantity 14th November.

Observing guage No. 2 had always the greatest quantity of rain, and No. 3 the least, on 1st January, 1847, I removed guage No. 2, to the situation of No. 3, and No. 3, to the place of No. 2, and these two years give the same result as formerly, showing it was not the guages, but the *situation* that gives the difference.

PETER MORRISON.

TABLE III.

TABLE OF THE FALL OF RAIN AT SYDNEY COTTAGE, GREENOCK, 25 Feet above the Sea.—By Captain M'KELLAR.

MONTHS.	1846.	1847.	1848.	Mean.
January,	3·3	2·7	3·5	3·16
February,	4·5	3·	12·	6·5
March,	6·4	1·4	4·	3·96
April,	1·7	3·5	2·5	2·9
May,	2·	2·8	2·2	2·33
June,	4·5	1·9	3·7	3·36
July,	7·	2·6	4·9	4·83
August,	6·1	2·8	4·1	4·33
September,	3·3	4·8	2·1	3·4
October,	5·3	6·9	4·8	5·66
November,	6·2	6·8	7·7	6·9
December,	1·5	6·6	6·7	4·9
Average,	51·8	45·8	58·2	52·23

Total Mean = 51·96

1846—Greatest quantity of rain in one day,—20th Nov.
1847— Do. do. do. — 6th Dec.
1848— Do. do. do. —22d Feb.

TABLE IV.

TABLE OF THE FALL OF RAIN AT GILMOURTON, AVONDALE. By MR. WISEMAN.

	1847.	1848.	Mean of Four Years, 1845—48.
January,	2·10	2·70	3·65
February,	2·10	10·85	4·61
March,	1·45	4·10	3·43
April,	4·	1·20	2·12
May,	4·10	1·20	2·42
June,	3·30	4·70	4·25
July,	2·10	3·90	3·72
August,	1·30	3·70	3·85
September,	4·40	1·85	3·63
October,	5·90	4·	6·70
November,	6·	7·	5·80
December,	4·59	4·60	5·22
	41·25	49·60	49·40

For Rain, &c., 1845—46, see Vol. II. page 139.

Dr. R. D. Thomson read the following paper:—

V.—*Analysis of the Yam.* By MR. JAMES PATERSON.

THE very great scarcity of potatoes induced many persons, during the spring of 1847, to use the yam, from its approaching nearer to the potato in quality than any other known root.

To understand the composition and relation between these bulbs, I made the following analysis of the yam, which, so far as I know, has not *been done previously*. For the specimen employed, which was of excel-

lent quality, I was indebted to Dr. R. D. Thomson. The experiments were conducted in the College laboratory under his superintendence.

Half a pound avoirdupois of yam was taken, after being deprived of all extraneous matter and skin, and was carefully rasped down and allowed to fall into a clean porcelain basin, previously weighed. The yam and basin were again weighed. The pulp was then thrown on a searce, and washed with distilled water till it passed through clear; the residue was then triturated and filtered, which diminished its bulk considerably.

On being tested by iodine, the starchy reaction was strongly marked. It was further subjected to a second trituration, and washed, when it still indicated the presence of starch, but in a much more faint degree.

The ultimate residue was then collected from the searce, dried at 212°, and weighed. It was then a hard, gray, woody looking substance, and had a faint resemblance to crumbs of very hard coarse sea biscuit, and was fibrous ligneous matter, and contained a mere trace of nitrogen, as was ascertained by experiment.

The solution, and that portion of the pulp which passed through the searce, was filtered through a weighed calico filter, and washed till the water passed through clear; what remained on the cloth was dried at 212°, and weighed; it was white and granulated, gelatinised when boiling water was added, and gave a bright blue with iodine.

The solution was then evaporated, and on being heated to the boiling point, a portion coagulated, being vegetable albumen, and on the addition of a little acetic acid, the quantity was greatly increased, the increase being vegetable casein. After being coagulated thoroughly, it was filtered and washed with difficulty, dried at 212°, and weighed. Colour dark gray.

The solution was then evaporated, and became a thick gummy substance; it was finally dried at 212° and weighed. It was then boiled for a length of time in water, to which some sulphuric acid was added, the water being replaced as it evaporated. As much chalk was then added as neutralized it. It was then filtered, and the solution evaporated, when there remained sugar, but having a brackish taste.

Specific gravity of yam employed was 1·1416. It contained 77·81 of water, 21·26 organic matter, and 0·93 of ash. By the above analysis, it gave,—

Fibrous ligneous matter,	3·51
Starch,	15·02
Casein,	2·25
Sugar and Gum,	0·73
Soluble salts,	0·75
Insoluble salts,	0·18
Water,	77·81
	100·01

Ultimate analysis, in the fresh state,—

Carbon,	8·19
Hydrogen,	1·32
Nitrogen,	0·39
Oxygen,	11·36
Ash,	0·93
Water,	77·81
	100·00

When dried at 212°,—

Carbon,	36·99
Hydrogen,	5·94
Nitrogen,	1·80
Oxygen,	51·14
Ash,	4·13
	100·00

From the preceding analysis, it would appear that the difference between the yam and potato is not considerable. The fibrous ligneous matter is greater in the potato than in the yam. In the analysis of Einhoff, probably a portion of the starch was not separated from the woody matter, as it is rather a difficult task; indeed, he terms it fibrous starchy matter. There is also more albumen and casein in the yam; but in the potato analysis, from the fibrous matter predominating, the albuminous matter must be deficient, as it is retained in the fibrous matter.

To ascertain if the fibrous matter contained any nitrogenous matter, it was subjected to analysis, and found to contain 0·161 per cent. in the fresh state, and 0·725 per cent. dried at 212°.

From the yam having more albuminous and nitrogenous matter, it is thus the most nutritious. Potato contains 1 nutritive to 9 calorifiant; yam 1 to 8½; and 17 pounds of yam are equal to 18 pounds of potatoes, being 6 per cent. in favour of the yam.

Mr. Simons exhibited an Electro-Magnetic Machine, constructed by himself.

28th March, 1849.—*The* PRESIDENT *in the Chair.*

MR. ROBERT SINCLAIR was elected a member.

Mr. Stenhouse exhibited and described a new oily body called Chloropicrine, obtained by boiling either charbazotic, oxypicric, or chrysammic acids with an excess of hypochlorite of lime.

Mr. Stenhouse also exhibited and described the properties of gyrophoric acid and its ether. Gyrophoric acid is the colouring principle of the *Gyro-*

phora pustulata and the *Lecanora tartarea*. Beta-orcine was also shown. It is the principle analogous to orcine, and is obtained either by boiling usnic acid with excess of alkali, or by subjecting it to destructive distillation. Likewise erythro-mannite, a sweet crystallizable body obtained from the *Roccella montagnei*; and quinto-nitrated erythro-mannite, the detonating compound analogous to quinto-nitrated mannite.

The Society afterwards resolved itself into a conversational meeting.

11*th April*, 1849.—*The* PRESIDENT *in the Chair.*

MR. ROBERT SINCLAIR and Mr. Howard Bowser, were elected members. The following paper was read:—

VI.—*On the Geological features of part of the district of Buchan, in Aberdeenshire, including notices of the occurrence of Chalk-Flints, and Greensand.* By WILLIAM FERGUSON, Esq.

THE general features of the district to which I purpose calling your attention, are those usually exhibited where the primary or crystalline rocks predominate: at the same time one or two interesting anomalies, are presented, or at least what presently appear anomalies, though a more minute investigation, and more extended observation of facts, may clear them up.

I shall attempt to sketch, generally, the features presented by the district, and then more minutely describe the peculiarities I refer to; the principal of these being the occurrence of a deposition of chalk-flints, and greensand.

Commencing our survey at the mouth of the Ythan, and proceeding northward, the coast line is very bold and precipitous, broken, however, here and there with creeks and bays. From the Ythan, the parish of Slains extends six miles along the coast. The average height of the rocks is from 170 to 200 feet, and they consist of gneiss and mica slate, with numerous veins of quartz; and at one part of the coast they are overlaid by limestone. I had occasion to traverse on foot several miles of this coast last summer. I approached it at the village of Collieston, where, in building the cottages of which it is composed, advantage has been taken of a ravine, which affords a comparatively easy access to the water. Part of the village is built on the water edge, and part on the cliff, 200 feet above. A very deep deposit of clay covers the cliffs, curling over them as it were, and presenting a steep slope covered with grass, leading to them. In some places the clay reaches to nearly the water edge, but in no instance, that I saw, touches it, an outlier of rock shielding it from the action of the water. In one place the overflow of

a burn had washed out a chasm in the clay, at least 30 or 40 feet deep, showing that the deposit is of very considerable thickness.

Between this spot, Collieston, and Ythan mouth, lies the old parish of Forvie. For many years this tract of country, extending some three or four miles along the coast, has been covered with sand to a great depth. The remains of the church walls were, even at a recent date, still traceable above the sand on the high lands near the shore.

Passing northwards from Collieston, we have the same high precipitous coast line for several miles, but so indented by creeks and narrow tortuous ravines, as to render the walk along the shore a very long one. Numerous caves are met with, some of them of great extent. Many of them enter from the sea, others are far above the sea level, indicating change in it. One of these latter I explored last summer. It was of considerable height, and after going about 45 yards into the rock, it descended abruptly.

On a high rock jutting out into the sea, stand the ruins of the ancient castle of Slains. To the north of it is a fine bay, with a beautiful sandy beach; but within a yard or two of the shore, numerous sunken reefs, and rocks just raising their ridges above the surface of the water, render the navigation of the coast very dangerous.

Not far from this point, still northward, is a very extensive cave called the "Dripping Cove." It differs from the last in that it occurs in limestone, and is filled with stalactites and stalagmites. I had often heard of it, and searched for it long and minutely, though in vain. It seems that the overlying clay, which is continuous all along the cliffs, has fallen in mass over its mouth, and completely shut it up. I examined all the brae, and climbed down to the sea level, and examined the rocks below. A stream of water, strongly charged with calcareous matter, was falling over the cliff, and covering the rocks with a limey incrustation. This water was actually percolating through the cave; but so completely is it now closed, that though, as I afterwards learned, I must have passed and repassed the very spot where it was, it yet remained undiscovered. Near this, where the clay reaches the edge of the cliff, it is fringed there with tall grass. When the culms have withered and fallen over the cliff, the water from the high ground above runs along them dropping from their points, and such is the perpendicularity of the cliff, falls from 100 to 150 feet into the water below.

As I have already stated, the principal rocks met with on this portion of the coast, are gneiss and mica slate. The next parish, that of Cruden, carries on the coast seven miles farther. The gneiss and mica slate, extend part of this way, after which there are two miles of a broad sandy beach, called the Ward of Cruden. The south end of this beach is marked by a remarkable reef of sunken rocks running out far into the sea, called the Scars of Cruden. It is terminated towards the north by precipitous cliffs of red granite, which extend from this point onwards beyond *Peterhead.*

There is little to be told of this part of the coast, further than a few descriptive remarks to exemplify how it has been disrupted and torn, and heaved into the ruggedest and most frowning coast line exhibited almost any where, indicating a "turgidum mare," and presenting a scene tallying to the "Infames scopulos Acroceraunia" of Horace.

On the first granite headland after passing the Ward of Cruden, stands the modern Slains Castle. It is almost insulated, a strip of sea running round to the north, and trending so far west as to leave only a narrow isthmus, affording access to the castle. This arm of the sea, called Langhaven, is narrow: in fact, is a mere rent or fissure on a large scale. It contains deep water, and the sides are so perpendicular and so high, that looking up from the water, the eye does not perceive a much greater breadth of sky, than looking down it perceives breadth of water. Seaward the cliffs are equally high and equally precipitous. It is said that from the library or drawing-room windows, a stone dropped falls directly into the water. It is recorded that there was formerly a carriage way round the castle. This is now gone, owing to the fall of a large portion of rock. Looking from the windows, nothing is to be seen but sea and sky.

Close by the castle there is a cave of peculiar construction. It opens to the sea below water mark, runs horizontally for a considerable distance into the rock, and then rises perpendicularly, till it comes to the surface in a field some way from the edge of the cliff. From the rolling of the waves into the cavern below, an atmospheric current is created, sufficiently strong to blow into the air any light article thrown into the upper aperture of the cave; and when there is a gale from the east, a column of spray rises continuously from it. This cave also, as well as the one formerly noticed, has received the name of Hell's Lum.* Indeed every cave of similar form, obtains this designation all over this coast.

Many isolated rocks of nearly equal height with the main line of coast, are scattered all along at various distances from the shore—one of these is called Dun Buy. Although Dr. Johnson says in reference to the urgent request of Lady Errol, that he should not leave Slains without seeing the Dun Buy, that there is nothing about it to detain attention, it is nevertheless, to those who see it, a very striking object. Description, however, can convey no idea of the peculiar feelings of awe and wonder, with which such effect of forces with which we are now unacquainted, cannot but be viewed.

The famous Buller of Buchan, is in this locality. On the north side of a little creek, presenting the usual perpendicular walls of immense height, the rocks jut out some way into the sea. In this promontory a huge circular pit has been scooped out. Its sides present perpendicular walls of rock, and towards the sea they are of comparatively inconsiderable thickness. At the top, at one place, not more than two or three

* Scot. for Chimney.

feet, but this only for a little space,—it is reckoned a feat to walk round. The sea flows in by a natural arch. In stormy weather, with an easterly wind, the dashing of the waves through this narrow aperture, and the recoil they make against the sides of the chasm, resemble the boiling of a huge caldron, and hence the name. It was a beautifully calm day when I was there. We took a boat and rowed round the point. We found the aperture below not much broader than admitted an ordinary sized boat. Even in the smoothest weather there is inside a peculiar roll in the water, and as the rock is caverned out in all directions, there is a hollow roar which adds very much to the sublimity of the scene. In the pompous language of Dr. Johnson, which is, however, well adapted for such a description as this, "we found ourselves in a place, which, though we could not think ourselves in danger, we could scarcely survey without some recoil of the mind. The basin in which we floated was nearly circular, perhaps thirty yards in diameter. We were enclosed by a natural wall, rising steep on every side, to a height which produced the idea of insurmountable confinement. The interception of all lateral light caused a dismal gloom. Round us was a perpendicular rock, above us the distant sky, and below us an unknown profundity of water."

Beyond Cruden the coast line extends about five miles, through the parish of Peterhead, commencing a little to the south of the point of Buchanness, and reaching beyond the town of Peterhead to the mouth of the river Ugie.

"Between the parish of Cruden," (I quote from the Statistical Report,) "and the fishing village of Boddam, in this parish, the sea is bounded by high cliffs of granite and other primitive rock, forming mural precipices: and this part of the coast is indented with many chasms, fissures, and caves, and these, in some cases, divide the granite from the trap rock. From Boddam to the bay of Sandford the coast is low and rocky. The bay of Sandford, extending some distance inland, is bounded by a flat sandy shore, intermixed with pebbles." Between the point of Salthouse Head and Keith Point, on which the town of Peterhead is built, the bay of Peterhead extends about a mile inland. Its shores are flat and rocky, terminating in sand and pebbles where it runs most inland. All this coast from Boddam to Peterhead, although low towards the sea, the rocks scarcely appearing above high water, except where the heads run out, and a sandy beach extending most of the way, is nevertheless abutted upon by cliffs of diluvium of considerable height, so that the general outline of the coast appears high. From Keith Point, which is the east-most of Scotland, the coast recedes to the mouth of the Ugie, preserving the same character of a rocky bottom, a sandy beach, and steep diluvial cliffs abutting on the sands.

"The whole of the parish of Peterhead," (I quote again from the Statistical Account,) "is upon primitive rock. In the Stirling hill, Blackhill, and Hill of Cowsrieve, the granite or syenite rises to the surface. Along the coast, and in other parts of the parish, it is covered

with clay, supposed to be diluvial, and other matters to a greater or less depth. Upon the Stirling hill the granite rises to the surface, or nearly so, over an extent of from 100 to 150 acres. In every place where the syenite or granite is laid bare, imbedded masses, veins, or dikes of primitive trap, gneiss, quartz, and compact felspar, are alternate with, and run through it. In some cases one half of a block is granite, and the other primitive trap, in complete cohesion, and often passing into each other. At the old castle of Boddam, the rock is separated by a fissure or chasm, one side of which is granite, and the other primitive trap. This chasm runs east and west, the granite being on the south, and the trap on the north, with a considerable angle to the horizon. Near the Buchanness lighthouse, there is a pretty extensive bed of hornstone porphyry, also a rock resembling clinkstone porphyry. The rock along the coast, from Buchanness to the mouth of the Ugie, may be seen at low water mark, and consists of granite, primitive trap, syenite, gneiss, compact felspar, felspar porphyry, and quartz, variously associated with each other. The Meethill is covered with a deep mass of diluvial clay: at the brickwork, which is about fifty yards from the beach, and where the clay has been cut to the depth of from thirty to forty feet, it exhibits various strata, which appear to have been deposited at different times, from their differences in quality and colour: some of the deposits are not above an inch in depth, while others are several feet. The skeleton of a bird was lately (1837) dug out of the clay here, at the depth of twenty-five feet from the surface, and about fifteen or twenty feet above the level of the sea." This diluvial clay, mixed in some places with rounded pebbles, covers a very considerable part of the parish.

When I come to describe the chalk-flints, I shall have to recur again to this portion of the coast, meantime I pursue my general sketch.

The next three miles represents the coast line of the parish of St. Fergus. The beach is flat and sandy, and the whole line of shore is thrown into two divisions by the rocks at Scotston Craig, each division forming a rude segment of a circle; the one extending from the mouth of the Ugie to the Craig, and the other onwards to near Rattray Head. The shore is completely cut off from the inland by a series of hills, which have been formed by the drifting of sand, and which being thickly covered with bent grass, prevent the sand drift encroaching on the rich arable lands of the interior.

The only rocks in situ, are to be seen at Craig Ewen, near the mouth of the Ugie, and at Scotston Head.

At Craig Ewen, we have granite, containing very little quartz in its composition, and exhibiting, though rarely, veins of compact felspar of a deep red colour.

At Scotston Head the rocks are accessible only at low water. They consist of granite, gneiss, trap, quartz, and limestone. "The gneiss and granite," says the Statistical Account, "appear often in close and *inseparable union.* The granite varies in appearance as it comes more

or less into contact with the gneiss: when the junction is complete it is white. When the granite underlies the gneiss, but without any union between them except contiguity, it assumes a dark colour, and discovers more hornblende in its composition than in its other positions. At one point the granite is graphic. The limestone is separated by a fissure from the granite, but appears in one or two places united to the gneiss; and there is reason to believe that it forms a junction with the granite at a more remote distance from the shore. At Hythie in the parish of Old Deer, and in a line due west from Scotston Head, limestone and granite of the same character as at the latter place, make their appearance in very intimate union. At Blackstones, between Scotston rocks and Craig Ewen, there are three distinct congeries of large boulders within the flood mark, consisting indiscriminately of granite, graphic granite, *primary and secondary limestone*, puddingstone, greywacke, gneiss, and basalt."

I have copied these sentences from the Statistical Account, and have retained the words primary and secondary limestone, because I found them there. I have, however, no evidence to give as to the distinction between the limestone, further than that the description denominated secondary, is said to contain ammonites and other shells, distinct from any of the known existing species.

In part of the parish, beneath the soil, the substratum consists of sand mixed with the remains of marine testacea. There are also indications along the coast that the land has been gaining upon the sea.

The parish of Crimond carries on the coast two miles farther. Beach and sand hills form the predominating feature, except at Rattray Head, where there is a long ridge of low-lying rocks called Rattray Brigs, running at right angles to the shore, and extending a mile and three-quarters in an easterly direction, into the German ocean. Great part of this ridge is only visible at low water. These rocks seem to consist of granite. Whinstone or trap, and also limestone, occur in various places. The principal feature of interest is the existence of a large loch, called the Loch of Strabeg. In 1700 this loch was of very small extent, and opened to the sea, so that small vessels could enter it. About 1720, a severe easterly gale blew up this communication with sand. The loch now covers an area of about 550 acres; and it receives all the streams of the neighbourhood. It has no outlet, and is wholly fresh. Its average depth is about $3\frac{1}{2}$ feet, and it is being gradually filled up by debris carried into it by the streams. I said there was no outlet, but it is very apparent that the surplus waters find their way to the sea, through the sandbank which separates the loch from the ocean. This belt, however, is about half a mile in breadth.

From Rattray point, four miles carries us over the sea board of Lonmay parish, a flat sandy beach, trending considerably to the westward. Two miles more cover the parish of Rathen, one point of which, that of *Cairnbulg*, runs out northwards into the sea, the coast line receding

again south-westward, so as to form a very considerable bay between it and Kinnaird's Head; immediately to the south of which last, lies the town of Fraserburgh.

The coast line of Frasersburgh parish extends about four miles. Two miles of this to the south of the town, are low and sandy. The rest is rocky but not high, except at Kinnaird's Head, which forming the turning point of the Moray Frith, stands out a high and bold headland. The rocks on the coast are gneiss and mica slate. Mormondhill lying to the south, in the interior, is quartz rock surrounded by gneiss.* Its height is 810 feet,†—at the upper end of the town of Fraserburgh, limestone occurs, and is quarried for building purposes.‡ Limestone also occurs in the parishes of Lonmay and Rathen.

Westward, the two parishes of Pitsligo and Aberdour, complete the district of Buchan and the shire of Aberdeen in this direction.

The coast line of Pitsligo is four miles in length. My impression of its appearance received from a hurried ride along the coast, was, that from Fraserburgh to Rosehearty it was sandy, rising in considerable hills, and at low water presenting low flat rocks beyond the beach. Onwards from Rosehearty towards Aberdour it is very different, rising the whole way in a continuous mural line of blackened and rifted precipices.

I staid two days one summer at Braco Park, about a mile west from Rosehearty. To wile away a forenoon we went to fish. The house was about a quarter of a mile from the sea. A single field lay between. Till within a hundred yards of the cliff edge the field presented a steep descent. At that point a little marshy hollow was carpeted with Anagallis tenella, or the Bog Pimpernel, and starred with the beautiful Parnassia palustris. Vaulting a three feet wall of loose stones, five or six yards more took us to the cliff. These are so precipitous that there is but one or two places here and there where it is possible to descend them. In descending we passed a fissure, going down plumb to the water, quite narrow, with equidistant sides, and in which the swell was roaring far into the earth with a hollow sound. This and numerous other fissures run farther into the cliff than the most adventurous ever yet penetrated. Of this particular one it is related, what, however, is said also of many others, that in it a curious explorer lost his life. He took with him the national musical instrument, bagpipes, that he might indicate to his friends on the earth, how far he had penetrated into it. It requires too great credulity to believe all that is told by the peasant, as to the length of time his music was heard, or the distance inland at which the decreasing strains were audible. One thing is certain, they ceased at last, nor did he ever return to tell how he had fared. About half way down the rocks, a broad plateau expanded, from which, by various perilous ways, it was possible to reach near the water, but at no point to attain it. Seated here upon a jutting out cliff, with feet hanging over the deep green water,

* *Nicol, p. 187.* † Statistical Account, p. 250. ‡ Ibid.

nought was to be seen but the wide expanse of ocean in front, unscaleable rocks on either hand, and behind, rugged precipices, our line of descent a-down their faces scarcely discernible. Westward, like a dim haze, rose into mid-air the Old Red Sandstone cliffs of Troup Head; the long roll of the Moray Frith, every now and then, sending a cloud of spray far up their rugged sides, as they stood out, as if it were in bold defiance or proud contempt of its impotent buffetings. The features of solitude are periodically changed during the season of the fishing; at least for an hour or two every evening, when the boats of Fraserburgh may be seen shooting out in crescent form from east to north-west; and those of Rosehearty stretching away to join them in an inner segment. It is beautiful to watch them as they gradually grow indistinct and dim in the distance, till the scene which was but now instinct with life, and that a life excited by all the perils of the deep, has been again resigned to the wild solitude and undisputed sovereignty of ocean. Such is a rude picture of this rock bound coast.

On the afternoon of the same day we rode along the coast as far as Aberdour. The same stupendous cliffs are witnessed. But the colour of the rock changes from the grays and blacks of the gneisses and mica slates, to the reds and browns of the Old Red. All along the coast deep glens run into the interior, so narrow and so steep in their declivities, that it is necessary to make the roads zigzag down the sides, and so up again. In these dens, as they are called, such as the Den of Aberdour, the Den of Auchmedden, the Den of Dardar, the climate is so mild, that stations for many of the rarer plants of our country are found in them. I only specify the beautiful Trientalis Europea. Along the coast caves abound. Several of these derive a deep local interest from their having afforded hiding places to Lord Pitsligo after the battle of Culloden. There is also to be seen at Pitjossie, a stupendous natural arch, through which the tide flows at high water, said in grandeur and magnificence to equal, if not surpass, the Buller of Buchan. But the astonishing feature of the latter spot is not the arch, but the pot into which the waters flow.

Gamrie, with its famous fish beds, follows Aberdour. On this I do not enter as being rather beyond the limits of my sketch. Beyond Banff at Boyndie bay the chalk-flints occur, as we shall see immediately when we trace the course and extent of this curious deposit.

We now return to the consideration of these chalk-flints.

Running slightly to the south of west there is a ridge of high ground taking its rise nearly at Buchanness, and stretching across the country continuously for eight to ten miles. At its eastern extremity it branches. One of the branches terminates south of Buchanness in the granitic mass already mentioned under the name of Stirling hill. The other ridge runs north of Buchanness, and may be said to terminate in the granitic escarpment of the Blackhills. All along the shore, between these points, *wherever the rocks* admit of a beach, quantities of water-worn flints

are found mingled with the other pebbles, evidently brought there by the waves. They are also found, although sparingly, on the southern ridge or Stirling hill. But on the Blackhill, and the neighbouring hill of Invernettie, the surface is almost covered with them. This ridge, at the distance of about seven-and-a-half miles from the sea, at Salthouse Head, attains an inland distance of about five miles from the coast opposite Slains. The flints are met with on the surface at various points along this line. The ridge is bare and moorish, covered with peat and heather, and this prevents the accurate tracing of the flints. At this point, however, seven-and-a-half miles along the line of the ridge, and about five miles from the sea, opposite Slains, they have been laid bare.

They occur at the extreme verge of the parish of Old Deer, and are principally seen on the farm of Bogingarry, on the lands of Kinmundy. The ridge of hill on which they occur, here trends to the north, coming round again towards the west, so as to expose to the south a deep bay, with a considerable inclination towards the south. The hill is crowned with moss and heather, part of which has been planted. The south face of the hill has, however, been under cultivation during the last twenty-five or thirty years. The flints are seen on the surface, commencing pretty far up on the east side of the hollow, and following at the same height the form of the bay, disappearing among the heather which has not yet been removed on the extreme west. They are in great abundance, covering a space of some twelve to twenty yards in breadth.

About 1830, in cutting a ditch to carry off the surface water from the garden of the farm house of Bogingarry, the bed of flints was come on, and found to be of considerable thickness. The ditch ran from south-west to north-east, traversing the flint bed; and a short cross one lay in the line of the bed.

When I saw the ditch first, it had been cut a good many years, and had become partly filled up. It had, however, a most singular appearance. It was crossed by the road to the house; and the water-run of the bridge was quite choked with rounded flints of all sizes. Above the bridge the bottom of the ditch was quite covered with rounded flints brought down by torrents. As you ascended the burn you could see the nature of the ground. The layer of soil was extremely thin, and below it the ditch was cut through a stiff yellow clay, scarcely a pure clay, more like a yellow clayey gravel, and so hard as to be pierced with extreme difficulty. Until you reach the bed itself very few flints are to be seen amongst the clay. The top end of the ditch and the cross one are in the bed. The flints lie closely packed together, imbedded in the already mentioned clayey matrix.

They wither when exposed to the air, becoming white, and in some cases, shivered. When newly taken out of the bed they usually break with a clear fracture, but they soon become hard and lose their facile cleavage. Every one contains some trace of organic remains. I have *examined a great many* and never missed seeing some indication of such,

although it is more rare to find them sufficiently perfect to make them worth preserving.

In the localities near Peterhead, there have been found "considerable varieties of the Echini family, occasionally entire, but more frequently only small portions of the impressions of these shells are found. Single spines frequently occur, and are distinctly marked. The Inoceramus, Pectens, and Terebratulae, are very abundant."

Flints are also found on the surface, on the hill of Skelmuir, adjoining Bogingarry. This hill is separated from that of Kinmundy by a valley, and a deep morass called the bog of Ardallie. South-westward they are found again on the hill of Dudwick, in the parish of Ellon. This seems to be their southmost limit.

I learn from a paper of Mr. Christie's, of Banff, published in the Edinburgh Philosophical Magazine, for 1831, that they occur, as already alluded to, at Boyndie bay in that shire; and also in a mass of diluvium covering the high grounds between Turriff and Delgaty Castle. The flints at Boyndie bay are found strewed along the shore, and contain traces of zoophytic organic remains. Those at Delgaty are likewise characterised by similar remains. The station at the latter place is ten miles from the sea, and is the highest ground in the neighbourhood. The flints are found in a mass of diluvial clay, cresting the hills. None are found in the hollows.

In my collection of fossils from Bogingarry and adjoining localities, there are impressions of portions of spines, and also casts of at least three varieties of the Echinus. There are also casts of Inoceramus, Terebratula, Pecten, Plagiostoma, Turbinolia, and Flustra, together with other remains not easily made out. From the remains in the flint, existing only as casts and impressions, it is very difficult, indeed it is almost impossible, to make out any of them with sufficient certainty to name them.

The other rocks in the immediate neighbourhood of the Bogingarry chalk flints, are granite, trap, and limestone. We have northwards, white granite at Smallburn, red at Newton and Greenmyre. The rising ground on which the house of Kinmundy stands, is a greenstone trap. Nothing but trap was met with in digging the foundation of the house; it was also met with along with a loose gravel below it, in sinking the well close by, 46 feet deep. Trap comes to the surface in the wood behind the house, and is quarried for dikes and drains. In the hollow behind, at Causey-ford, we have a deep deposit of peat. On the south side of Millhill, granitic gravel. On the north side, granite quarried for building purposes. Below Barnyards on the burn side, we have a sort of mica slate. West from that, above the hills of Coynach and Knock, there are immense boulders of clinkstone. These are water-worn: some of them are many tons in weight. Four miles further, at Hythie, limestone resting on granite.

North-westward, at Annochie, we have limestone quarried for burning. *It is much cut by* veins, dikes, and blocks of gneiss, from which we

may gather it rests there upon gneiss. It is impure, containing a good deal of magnesia. Beautiful crystals of Iceland spar are met with in drusy cavities in the rock.

The country presents numerous simple minerals. Many varieties of quartz, such as milk, rose, violet, ferruginous, spongy-form, &c.; and sometimes very large specimens of rock crystal are picked up in the fields. Jasper is common. Veins of antimony are found in the granite, as are also some ores of iron in small quantities. Manganese in the dentritic form, is seen sometimes in the limestone. Crystals of schorl, sometimes of large size, I have often procured in huge fragments of white quartz. In one spot there is a quarry of these quartz blocks, some of which are of great size. They are not water-worn. I once picked up a piece of granite covered with crystals of Beryl.

In the peat are found trunks of trees, principally oak; and large quanties of birch and hazel, with nuts of the latter. Not a hazel bush has been seen in the district for upwards of a hundred years, yet in some places, by simply turning over the turf, hundreds and thousands of hazel nuts may be laid bare. The antlers of stags have also been dug up in the district, but not recently.

I must now call your attention to another geological feature of very peculiar interest. It was stated by Dr. John Sheir, to the natural history class, Mareschal College, 1839, that the greensand was said to exist in Cruden. * I have caused as particular an examination to be made as was possible at such a distance from the spot, and I have received specimens which I have submittted to Mr. Bryce, and which entitles us to say that the greensand is found in Cruden.

The deposit in which it occurs seems to run through two parishes, Slains and Cruden, and differs in its lithological character in different places. My specimens are from two points, six or eight miles distant from each other. The first or southmost point, is about two miles inland from Collieston. The deposit takes the form of a ridge of hills surrounding three sides of a loch. This loch has been ascertained to be in one place fifty-two feet deep, and the hills rise around it to a height of from forty to fifty feet. They are composed of gravel mingled with comminuted shells, and containing water-worn nodules of limestone, mica slate, and gneiss. The limestone nodules contain organic remains. Among the specimens sent me, only one contains organic remains; I cannot, however, determine what they are. I have also received several specimens of the sand and gravel among which the nodules occur. After removing the recent helices, there still remains the debris of broken seemingly marine shells. The nodules are much water-worn. Although I describe this portion of this curious deposit in this place, it by no means answers to the character of greensand.

* This fact was communicated by the late Dr. Knight to Dr. Thomas Thomson *nearly twenty years ago.*—EDIT.

The deposit at the other point, namely, Moreseat, was said to consist of a calcareous sand, visibly stratified, of a greyish hue, and also composed of comminuted shells. This I have also had examined, and have received specimens of a grey friable sandstone, not water-worn, and containing shells of various genera, such as Cardium, Terebratula, and Trochus. My informant writes of these:—"I went to Moreseat and got some shells in a kind of sandstone. The stones they are to be found in, are in a broken state, among clay of the same colour as the stones, with another substance I have sent you." This seems to be a fuller's earth. He does not say any thing of the stratification, nor of the calcareous sand. From the friable texture of these specimens, and the fragile nature of the enclosed shells, it appears obvious that the deposit either is in situ, or at least cannot be far removed from it. Several of the specimens are well marked with the small green grains of silicate of iron, or chlorite, which has given the name of greensand to this the lowest member of the cretaceous group. It is seen at Moreseat in two places. It was first discovered in digging a pit for the water wheel of a threshing mill. It is nine feet below the surface of the soil, and seemed deep, but is now covered up. About 400 yards from this point, it was also opened up in making a ditch, and there it is only three feet below the surface. The locality is on the side of an eminence. There is a thin layer of a brown substance like fuller's earth. The shells are in fragmentary sandstone nodules, all presenting a broken appearance, and mixed with a half sandy half clayey substance of the same colour. The fragments are very soft when newly dug, but harden on exposure to heat, and turn lighter in colour in drying. My specimens were dug from the bottom of the ditch, and with difficulty, for the water filled the hole very fast. This prevented very accurate observation of the nature of the bed.

It may not be uninteresting now to review what has been observed and theorized, with respect to the formation of flints. They occur in the highest bed of the cretaceous group found in this country, which from their presence has been named the chalk with flints. It covers a very large portion of the south-west of England, reaching as far north as Flamboroughhead,* but has not been satisfactorily proved to exist in Scotland.† "The chalk of this subdivision," (says Dr. Mantell, Geol. Sur. of S. E. of England, p. 73,) "is generally of a purer white, and of a softer texture, than the inferior strata, but in other respects presents no sensible difference. It is regularly stratified, and partakes of the general inclination of the other divisions of the series. It is separated by horizontal layers of siliceous nodules, into beds that vary from a few inches to several feet in thickness, and which in some localities are traversed by obliquely vertical veins of tabular flint, that may be traced for many yards without interruption. These are sometimes disposed horizontally, and form a continuous layer of thin flint of considerable extent. The

* *Ansted, I.* p. 456. † Ibid. p. 458.

nodular masses of flint are very irregular in form, and variable in magnitude; some of them scarcely exceeding the size of a bullet, while others are several feet in circumference. Although thickly distributed in horizontal beds or layers, they are never in contact with each other, but every nodule is completely surrounded by the chalk. Their external surface is composed of a white opaque crust, consisting of an intermixture of chalk and silex, probably formed by a combination of the outer surface of the nodule with its investing matrix, while the former was in a soft state. Internally they are of various shades of gray, inclining to black, and often contain cavities lined with chalcedony, and crystallized quartz."

By the analysis of Klaproth flint, consists of

Silex, ...	98.
Lime, ...	·05
Alumina, ...	·25
Oxide of Iron, ...	·25
Water, ...	1· *

It is infusible. Submitted to a great heat it becomes white and opaque. In withering it assumes various colours; becoming most frequently either red or yellow.

It seems established by minute and extended observation, that each flint is formed round some organic nucleus. Apparently these were in all cases at first sponges; and the presence of plates and spines of echini shells, &c., is accounted for by supposing that the sponges grew through and over such.† The silex must have been held in solution in the waters from which the chalk was deposited. I believe no chemical solvent of silex, has yet been discovered. It is stated, however, that steam at a very high temperature dissolves it: the temperature required being a little above that of fused cast iron.‡ We know that it is held in solution by many thermal springs, especially in the Geysers of Iceland, ‖ and in thermal springs in the neighbourhood of the volcanic mountain of Tongariro, New Zealand. §

Dr. Buckland's theory, as published in the fourth volume of the Geological Transactions, is simply this,—he assumed that the whole mass was, previous to consolidation, a pulpy fluid, and that the organic bodies found in the flints, were lodged in it before the separation of its calcareous from its siliceous ingredients: that then these organic bodies became nuclei, to which the flint, upon the principle of chemical affinity, attached itself. ¶

Mr. Bowerbank has examined, with great care, the flints of the upper chalk, and he has arrived at the conclusion, that in all cases the siliceous matter has formed itself on organic bodies; and that these organic bodies

* Mant. Geol. Sur. of S. E. of Eng. p. 74. † Ansted, I. p. 474.
‡ Rep. Brit. As. 1840. Jeffrey's Exp. ‖ Hooker's Trav.
§ *Dieffinbach.* ¶ Mantell, Geol. Sur. of S. E. of Eng. 77, 78.

are sponges.* His main results given in Mr. Ansted's words are these, "It would seem as if the sponges, which under certain circumstances had grown and flourished at the bottom of the sea, had been at successive periods covered up by fine calcareous mud, deposited gradually upon them; that the particles of fine mud, thus sinking gently from suspension in the water, had barely penetrated below the surface of the sponges, resting upon them, but not flattening them by the pressure: and finally, that owing to some chemical cause, a deposit of siliceous matter being in progress, the particles of silex were attracted to these organic bodies, themselves containing some portion of the same mineral. It appears that we can in this way, and no other, account, with any degree of plausibility, for these three phenomena, viz:—first, for the existence of beds of flint in the chalk: secondly, for the organic structure visible in the flints, and the frequent occurrence of fragments of corals, shells, and zoophytes imbedded in them: and lastly, that in some cases large silicified sponges are found growing vertically, one upon another, to a height of several feet in the chalk; and that sponges, now silicified, have often grown through and over the shells of echini, or molluscous animals, and even of other sponges.†"

These theories of Dr. Buckland and Mr. Bowerbank, of which I have given you an abstract, seem to be nearly all that is known about these flints in their natural position, in beds in the geologic scale. They are, as Mr. Ansted remarks, equally puzzling to the geologist, the chemist, and the zoologist. But when they are found, as they are in Buchan, overlying the granite, they form a geological problem as hard to solve as their own substance.

From our brief survey of the surrounding country, we saw that the predominating rocks are the crystalline, and the stratified unfossiliferous. Only in one instance did we find a limestone, with organics, (an ammonite) which might consequently belong to the secondary group. Old Red Sandstone occurs at Aberdeen, again (certainly) at Gamrie, but it has not been positively seen at any point between, although it has been supposed that it may nevertheless envelope the primary rocks along the coast, beneath the sea level. Oolite and Wealdenbeds occur in the neighbourhood of Elgin. The distance between these beds at Llanbride, and the flints of Buchan, cannot be less than between fifty and sixty miles. Water-worn fossils of the lias occur at Blackpots, near Banff, but there they are manifestly in a diluvial clay. The Old Red Sandstone is the newest rock that is known to occur over all Banffshire; consequently the whole of this county comes between the deposit under consideration, and the newer formations of Morayshire.

This newly determined greensand of Cruden, is the only rock at all approaching, in the geological sequence, the chalk beds from which the flint boulders must have been derived. We are forced to conclude concerning it, that if it is not in situ, it is at least not far removed from it.

* *Ansted, I.* p. 472. † Ansted, I. pp. 474, 475.



The question then arises, How came the flints there, and whence?

Mr. Hugh Miller, cautioning the young geologist against concluding that because he finds a rock resting upon gneiss, it is therefore low in the geological scale, instances, as an example of the error such a conclusion would lead to, the flints and chalk fossils of Banff and Aberdeen, lying immediately over it in these counties, and adds, "It is probable that the denuded members of the cretaceous group once rested upon it, there."* Mr. Jamieson too in the Edinburgh Philosophical Journal states the same opinion, adding, that it will probably be found in some of the hollows of this part of Scotland."†

This is one theory. That the lower beds, and the chalk of this very bed itself, have been removed by denudation, leaving the flints resting on the granite.

Opposed to this theory is the fact, that the flints are invariably water-worn. True, even according to it they would have presented such an appearance, but not necessarily to such an extent, and it seems that a denuding agency sufficiently powerful to produce the rolled effect noted, would have removed them as well as the other beds, especially as they occur *not in hollows*, but always on the sides, and near the summits of hills.‡

Mr. Nicol states his opinion thus:—"Probably these recent secondary formations once existed here, or may still be covered by the sea, and connected with the similar beds on the Moray Frith. This opinion is confirmed by the occurrence of lias, containing coal at Hogenaes, in the south of Sweden, where it rests on gneiss and is covered by chalk."‖ This leads on to another theory which has been suggested to account for these flints, namely:—That, however such secondary beds may have once existed here, these individual water-worn flints owe their origin to a transporting agency, which has brought them from the chalk formations of the northern continent.

The volcanic and tidal agencies, (the latter modified by local currents,) assume a direction between south-west and north-east. All the mountain ranges and great formations of our island assume, in general, that direction. The great mountain range of Norway assumes the same. I am too unacquainted with Norwegian geology, to be able skilfully to connect it with Scottish. At Christiania there is a group belonging partly to the lower, and partly to the upper silurian rocks.§ True chalk with flints has been clearly determined in some parts of Denmark.¶ This Danish group may have been continued into Norway at one period, and afterwards removed by denudation, the same agency transporting the flint nodules to our own shores.

It may bear against such a supposition of transportation, that the direction of the currents seem usually to have been *from* south-west *to*

* Old Red Sand. 3d. Ed. p 262. † 1831, Vol. 10, p. 163.

‡ A few individual specimens are found scattered over the hollows; the mass, however, as stated in the text, is invariably on or near the tops of the hills.

‖ *Geol. of Scot., p. 188.* § Ansted, I. p. 118. ¶ Ib. p. 461.

north-east, and that for this theory they required to have been reversed. It may be suggested, Might not the elevation of the great northern mountain ranges of the continent, have been sufficient to cause a current from its shores, capable of exercising the transporting power required? The presumption is, however, against such a supposition.

Standing on the ridge of the hill of Kinmundy, and looking towards the south and east, there is spread out before the eye a wide expanse. Slightly to the north of eastward, the ridge is continuous to the sea at Buchanness. Westward it undulates, receding northward, and again stretching out a promontory to the south. Beyond this there is a gorge, narrow and deep; and again the hills rise, stretching away westward and northward, running out in a series of high grounds by Dudwick towards Turriff and Delgaty, and so onwards to the sea at Boyndie. Between this ridge and the sea, on the south and south-east, there stretches out from the sort of bay described, a breadth of five or six miles of levelish country, presenting inequalities of surface but in the main level, till it reaches the sea with a coast line elevated 180 to 200 feet above the sea line. It is over this valley that the calcareous sands occur. It is near its centre that the greensand formation lies. And standing, as I have said, on the hill ridge, and marking, as one cannot fail to mark, the band of flint boulders that line near their highest, and at an equal elevation the various bays and promontories, it requires no great stretch of imagination to conceive of the waves of the German Ocean as having once rolled even hither, bearing with them, and depositing on their innermost bounds, the rounded flints that now mark their ancient shore.

But it may be argued, the greensand beds lay right in the way, and must have suffered also from the denuding power of the waves. If future examination shows these beds to be in situ, we must yet look for another theory.

I have already stated that the shores of the little bays near Peterhead, present large quantities of the rounded flints. These may be either brought down by streams, or cast up from the sea. I have also inferred from the condition of my specimens of organic remains, from the Cruden greensand, that that formation is either in situ, or at least not far removed from its original position; not presenting evidence of being water-rolled, and not capable of undergoing, without destruction, that process.

I wish to connect these two facts with an idea hinted at by Nicol, as already quoted, and additional grounds for which have been pointed out to me by Mr. Hugh Miller. Across the southern districts of England, we have a certain sequence of geological formations, including in regular succession the lias, oolite, and wealden, succeeded by the cretaceous. Across that portion of Scotland immediately to the north of the district at present under our consideration, we have part of the same sequence commencing with the lias. This formation at Cromarty and at Brora, in Sutherland, is considerably to the west of the first appearance of the same formation *in England; but this* results naturally from what was before mentioned

of the geological formations, running not east and west, but north-east and south-west, not right, but diagonally across the country. We have thus lias at Cromarty, and a lower oolite near Elgin. May it not be possible that all we want to complete the remaining members of the series, is simply to be able to carry out our section into the Moray Frith?

Such an hypothesis receives confirmation from the fact, that in the neighbourhood of Elgin, are beds containing wealden fossils, "which," says Nicol, "we are led to suspect are not original formations, but fragments of more extensive beds, perhaps drifted to this place."* The diluvial clay containing lias fossils at Blackpots, also may indicate a formation beneath the water of the bay. By referring to the geological map of England, it will be seen that the greensand accompanies the chalks lying on the west of it, and on the east of the lias, to the shore of the channel. Our patch of it at Cruden, might form part of the termination of a similar stripe, unless it too can be accounted for in the same way as the Moray wealdens, by supposing it a drifted fragment from the north.

May we then fairly infer, that at one period the space now occupied by the Moray Frith, contained a perfect sequence of the secondary formations? That first, the soft chalk strata suffered denudation by the ordinary action of north-easterly gales, currents, and drift-ice, and that the roll of the German ocean piled up its debris in the shape of these water-worn flint boulders, along its successive ancient shores, and that the wealden and oolite of Elgin, and lias of Blackpots, followed in the same course?

That part of this theory applicable to the lias of Blackpots, Mr. Miller states thus, in his description of that deposit:—

"There had probably existed to the west or north-west of the deposit, perhaps in the middle of the open bay formed by the promontory on which it rests,—for the small proportion of other than liassic materials which it contains, serves to show that it could be derived from no great distance, —an outlier of the lower lias. The icebergs of the cold glacial period, propelled along the submerged land by some arctic current, or caught up by the gulf-stream, gradually grated it down, as a mason's labourer grates down the surface of the sandstone slab he is engaged in polishing: and the comminuted debris, borne eastward by the current, was cast down here."

At Blackpots, the lias fossils occur in clay containing few other boulders. At Boyndie, further west, the flint boulders cover the shore; and at Delgaty, ten miles inland, they occur in great abundance, along with boulders of quartz rock, but no fossils except their own. It would therefore appear that we owe the flint boulders and the lias boulders to different periods. And as the chalk overlies the lias, it may be that its denudation was completed, and its fossils thrown up upon the high grounds of the interior, previous to the formation of the boulder clay, containing the fossils of the lias. Although apparently not here, the boulder clay has in other places, (as on the banks of the Thorsa, in Caithness,) been found to contain "fragments of chalk-flints, and also

* P. 193.

a characteristic conglomerate of the oolite," as well as comminuted fragments of existing shells. These facts seem also to favour this hypothesis.

The subject altogether is one involved in considerable darkness, and it is perhaps vain to attempt any generalization upon it, till the local geology has been far more accurately examined and determined. This has not hitherto been attempted. A primary country is thought to present a barren field, and is too often passed over as devoid of interest, whilst it often happens, that from want of the proper survey, most interesting facts are overlooked. We may yet find in primitive Buchan, a beautiful geologic sequence. We have granite and trap, and gneiss with its accompanying slates, we know. We have limestone, and some trace of the Old Red. We have also a report of a fossiliferous limestone, and an alleged greensand and chalk. The surmises on these subjects may become facts, and the investigation requisite to make them so, must lead to new discoveries. But proved or disproved, much of interest to the geologist, both scientific and economic, cannot fail to come to light in the examination, and the labour which must be expended on the process will not be thrown away.

25th April, 1849.—*The* PRESIDENT *in the Chair.*

MR. HOWARD BOWSER was admitted a member. Mr. Keddie gave in the following

Report from the Botanical Section.

19th December, 1848.—Dr. Walker Arnott made some observations on the position of the carpels in the bicarpellary orders. The usual mode of ascertaining this is by inspecting the flowers in a growing state, and observing whether the two carpels are superior and inferior, or right and left with regard to the axis of the *plant*, or with regard to the subtending bracteas; but from the *peduncles* and *pedicels* having often a tendency to twist, this method may lead to innacurate results. Dr. Arnott then referred to the spiral disposition of the leaves on the stem and branches, and of the theoretical arrangement of the verticels of the flower; and showed that in all cases one of the *sepals* (when there were *five*,) was either superior or inferior: when, therefore, the two carpels were also superior and inferior, one must be placed opposite to the centre of a sepal, the other between two sepals; when, on the other hand, the carpels were at right angles, or right and left, a line passing through them would cut the sepal on the one side precisely as it did the one on the other. By attending carefully to this, an isolated flower would be sufficient to enable one to determine the position of its carpels with regard to the axis of *inflorescence*. Dr. Arnott mentioned that at least one group, reputed to *have its* carpels superior and inferior, has them in some genera the contrary way, so that the whole subject requires revision, before we can place

dependance on this character for distinguishing alliances and even natural orders. It is, however, one that can only be successfully followed out by those who reside in tropical countries.

27th February, 1849.—Mr. Gourlie read an account of a Botanical excursion to the Breadalbane mountains, in July, 1841, and exhibited specimens of the plants of that district, amongst which were the following:—

Arenaria rubella.	Erigeron alpinus.	Salix reticulata.
Saxifraga nivalis.	Bartsia alpina.	Aspidium Lonchitis.
cernua.	Rubus Chamæmorus.	Woodsia hyperborea,
Dryas octopetala.	Cerastium alpinum.	Myosotis alpestris.
Carex atrata.	Potentilla alpestris.	Hypnum Halleri.
saxatilis.	Juncus biglumis.	Saussurea alpina.
capillaris.	triglumis.	Arbutus uva-ursi.
Draba incana.	castaneus.	Sibbaldia procumbens.
rupestris.	trifidus.	

18th April, 1849.—Dr. Walker Arnott read the following paper and exhibited specimens of the plants collected.

VII.—*Account of a Botanical Excursion to the Rhinns of Galloway.*

By G. A. WALKER ARNOTT, LL.D., Reg. Prof. of Botany.

ON Monday the *7th August* last, as previously arranged at a meeting of the Botanical section of the Society, Dr. R. D. Thomson, Mr. George R. Alexander, Mr. William Kidston, and I, started by the railway for Ayr, and from thence took the steamer to Stranraer, where we arrived between eight and nine in the evening, too late to proceed further that night. By means of a car next morning, we got to Drumore to breakfast, a long ride of three hours. In this ride we passed Stoney Kirk, where we observed *Lythrum Salicaria*, and *Equisetum limosum*, now known by the name of *E. Telmateia*—Chapel Rosen Bay where we saw *Sparganium simplex*, and between which and Grenan Craigs, *Lepidium Smithii* first made its appearance. By many *Sparganium simplex* is confounded with *S. ramosum*, but its simple inflorescence affords a ready mark of distinction. The history of *L. Smithii* may not be uninteresting. It was first described by Sir J. E. Smith under the name of *L. hirtum*, on the supposition that it was the *Thlaspi hirtum* of Montpellier, a South of France plant, which agrees with it in the length of the style, and in the nearly total absence of scales on the pod, but differs widely in the form of the pod and its hairiness, besides other characters. Hooker, in the Flora Scotica, expressly says that the pod is not only free from scales, but also from hairiness, and although the hairiness is characteristic of the true *L. hirtum*, he retains that name: it is probable that at the time (1821) he was unacquainted with the true species. In 1823 I first obtained the Linnæan plant, and then noted in my copy of the Flora Scotica, the differences between it and the English one; when Sir W. Hooker prepared the British Flora in 1830, he changed the name to *L. Smithii*, having ere then received from myself and others,

the *Th. hirtum* of Linn., which is not found, so far as I know, beyond the region of the olives: he made the remark also, that the English plant seemed quite unknown on the continent. In 1825, however, I met with an allied plant in the Pyrennees, which interested me a good deal: we found it in the Vallée d' Eynes, on the 24th June, quite glabrous: at La Massane, in the republic of Andorra, on the 9th July, with the leaves glabrous, and stem hairy: and at Mont Louis on the 29th June, with both leaves and stem hairy, and in no respect distinguishable from the English plant. That this species, scattered over the Pyrennees, could not be unknown to De Candolle was highly probable, and it agreed so well with his *Thlaspi heterophyllum*, as to leave no doubt on the matter. Bentham in his Cat. des Plantes des Pyrenées et de Bas Languedoc, published in 1826, placed it in its proper genus, and gave it the name of *Lepidium heterophyllum*, characterising it as well as *L. campestre*, and *L. hirtum*. He did not, however, advert to the Mont Louis hairy specimens, and speaks only of the glabrous leaved form; this prevented him comparing it with the English and north of France plant, which in his remarks under L. hirtum, he says, seemed only a variety of *L. campestre*. While drawing up some notes connected with my excursion to the Pyrenees, I was led to reconsider the whole subject, and in August 1829, I published a note in Jameson's Journal, (p. 321,) pointing out that there was no essential difference between the English plant and the Pyrenean one, and to that opinion I still adhere. The name *L. heterophyllum* is thus the older one by four years, and ought to be retained, although it must be confessed the other is always more likely to be adopted in this country. I have no specimen from the north of France, but I have no doubt whatever that it is as common there as in this country. A small plant, *L. humifusum*, from the mountains of Corsica, described by Requien in the Ann. des. Sc. Nat. v. p. 385, I can scarcely distinguish from the Pyrenean glabrous one.

On arriving at Drumore, our driver took us to what he believed the best inn; but as we had been directed to go elsewhere, we, after making an inspection of the premises, returned to the car, and drove to another. In this we punished ourselves, for unquestionably the driver was in the right, as far as cleanness was concerned; and as for accommodation, they were on a par, for none of the magnificent hotels of that town can boast of more than a couple of beds for strangers. In the one we went to, on the shore, they could only accommodate one half of our small party, two having to seek nests elsewhere; and to add to our misfortune, the clean inn we had first been at, now refused them admittance, no doubt offended with our deserting them: and as it is not the custom in that primitive place, to charge for lodging when one breakfasts or sups in the house, the good people having been baulked of us in one way, had reason on their side in refusing us mere lodging, for during the day one or two visitors might possibly (though not probably) turn up. This leads me to observe, that any excursion to the Mull of Galloway, in which the *party consists of* more than four, will find themselves too numerous, if less

than that number, the expenditure for a car will be rather heavy, unless when they have abundance of time in hand, and choose to walk, or wait the time of starting of the Stranraer, Drumore, and Port Logan coach, when unquestionably two will enjoy more comforts than any greater number.

After breakfast we started for the Mull, along the coast. The distance by the road is probably only about five miles, but we found it increased to nine or ten by following the sinuosities of the shore.

Not far from Drumore, in the bay, we found *Glaucium luteum* rather scarce, *Polygonum Roberti*, *Atriplex laciniata*, and another *Atriplex* which presented so many different appearances, that I could make nothing of them, even by the aid of Babington; some were erect, some prostrate, some assurgent: and the form of the perianth (or bracteas, as Moquin Tendon, perhaps properly, considers them,) of the female flower was so variable, that I was forced to come to the conclusion that all were states of *A. patula*. As to *Polygonum Roberti* Loisl. (the *P. Raii* of Bab.) it is known very readily from *P. aviculare* by its pale coloured patulous perianth, and large dark coloured shining seed-vessel, which projects considerably beyond the perianth. Mr. Hewett Watson, has lately suggested that it is a mere variety of *P. maritimum*, a subject on which I can scarcely pronounce an opinion, from dried specimens, and those that I myself collected of *P. maritimum* abroad, were too little advanced to enable me to judge of its striking first-sight character. The difference, however, indicated in our books is far from satisfactory, and is taken from what we know to vary in other allied species. It is singular that Sir W. Hooker in his two last editions of the British Flora, says, that the fruit is *shorter* than the perianth, although in the former editions, and in the English Flora, it is characterised (as var. β of *P. aviculare*) by fruit *longer* than the perianth. The British *P. maritimum*, (from Jersey) may possibly be only a form of *P. Roberti*, and different from the true one of the south of Europe: of it I am not sure if I possess specimens, (if so they are at present mislaid): the one from the Mediterranean has glaucous leaves, which blacken by drying, and short joints in proportion to the stipules. My friend Meisner, in his monograph, says of the *P. maritimum*, that it is found also in the west of France, North America, Cape of Good Hope, and perhaps in India. I fear he has jumbled several things together; and the west of France plant may be either our *P. Roberti*, or the Jersey *P. maritimum*. I do not seem to possess any thing *exactly* agreeing with *P. maritimum* as *defined* by that botanist, except from the Mediterranean. The *P. aviculare* is a very variable plant, and has a wide geographical distribution.

At Killiness point is a large bank of drifted sand, held together by *Psamma* (or *Ammophila*) *arenaria*, and *Triticum junceum;* here the *Calystegia Soldanella* is found, and also *Anacamptis pyramidalis*. This I believe to be the only certain spot in Scotland for the latter, and the locality is extremely interesting, in so far as it differs entirely from that usually assigned it, viz.: "grassy hills or banks, especially where the soil is *chalky.*"—*Sm.* *We saw* a considerable number of specimens past flower;

these of course we left: only about a dozen were obtained in a good state. The coast instead of a south-easterly, takes now a south-westerly direction to Mary-port, nearly half-way between Killiness point and Mull farm. At Mary-port we met with *Raphanus maritimus*, very luxuriant, but apparently passing into *R. Raphanistrum*, unless indeed both were growing intermingled, and forming hybrids. The strangulated or beaded appearance of the fruit has always appeared to me the best character, although that has been departed from by most botanists, who lay more stress on the lyrate, or simply pinnatisect nature of the leaves; to this latter I object, as it does not seem of much value in other species of the Cruciferæ, but I by no means insist on the validity of that obtained from the fruit, for reasons best illustrated by the specimens on the table. Near Mary-port the *Helosciadium nodiflorum* was found, and *Samolus Valerandi*, the first time we had seen it on this coast.

Carlina vulgaris was got on a sloping bank vis-a-vis to the Mull farm house. From this point to the lighthouse, we observed nothing worthy of notice: and no sooner had we got to the lighthouse, than a cold drizzle came on, which made our scrambling down among the rocks too hazardous to be attempted: we got here a boy accustomed to them, to descend for the *Crithmum maritimum*, and *Inula crithmoides:* the former was not sufficiently advanced, for this, like all other Umbelliferæ, ought to be collected when the fruit is almost mature. This plant is not so scarce in Scotland as supposed, extending from the Mull of Galloway to the parish of Kirkbean, on the east coast of Kirkcudbrightshire: it is also found to the south of Ayr. The Golden Samphire, *I. crithmoides*, is more rare. To dry both I found it necessary, on my return to Glasgow, to scald them in boiling water, not only to kill them, but to take the salt out of them, which last, if not extracted, ruins specimens that are not kept constantly in a perfectly arid atmosphere. We could not obtain a boat, and therefore we found neither the *Atriplex portulacoides*, nor *Statice spathulata*, found here, as I understand, by Dr. Balfour and his party, in 1843. I was rather disappointed at this, because I never happened to see, in a growing state, the *Statice*, so called in this country, although it appears to be by no means uncommon. My attention was many years ago directed to probably a form of it as distinct from *S. Limonium* by Mr. M. Y. Stark, now a clergyman in Canada, who found both on the south shores of England, and observed that the *S. spathulata* uniformly grew in circles or rings, like fairy rings, while the other never did. As to the name this plant ought to bear, it seems tolerably certain that it is not *S. spathulata* of Desfontaines,—indeed, Boissier, the latest writer on the subject, places them in different sections of the genus; it appears to be the *S. bellidifolia* of most French botanical writers, but not of Sibthorpe's Flora Græca, nor of De Candolle: it is the *S. oleæfolia* of Willdenow, but not of Scopoli, and it is the *S. Willdenovii* of Loisleur in part only, the specimens he had chiefly in view being the *S. densiflora* of Girard, a Mediterranean plant: indeed until

Girard attempted to clear up the point, not one name given to it was correct. Girard, therefore, (in the Ann. sc. Nat. N.S. xvii. p. 31.) gives it a new appellation, *S. Dodartii*, and indicated two varieties: his β *humilis* was supposed to be peculiar to the Mull of Galloway, while his α was not uncommon along the west coast of France. Since then the *S. Dodartii* has been divided into two—*S. Dodartii* and *S. occidentalis*; and if distinct, it is not impossible we possess both in Great Britain. The true *S. Dodartii* is said to have leaves obovato-spathulate, awnless or very shortly mucronate, and tapering into a petiole about as long as the limb; the scapes are rigid, straight, without a trace of sterile branches; bracteas green on the back, with a narrow whitish margin, and anthers somewhat linear. *S. occidentalis* has leaves lanceolato-spathulate acute, or with a long setaceous mucro below the point, tapering into a petiole that is longer than the limb; the scapes are slender, flexuose, with some of the lower branches sterile; bracteas with a shining but reddish margin; anthers ovate. Such, at least, are the characters furnished by Boissier, in De Candolle's Prodromus, just published: he mentions that *S. Dodartii* grows in England and Ireland, on the authority of a specimen from Hooker; but states that the *S. binervosa* of Gerard Smith, judging from the figure in Engl. Bot. Supp. t. 2663, is rather *S. occidentalis* than *S. Dodartii*, on account of the form of its leaves. Now, I lay before the Society all my British ones, and they will see that every one has the lanceolate-spathulate leaves of *S. occidentalis*, although some of the other points in its character do not apply. On the other hand, Boissier refers, with doubt certainly, to *S. spathulata* of the Bot. Mag. as a figure of *S. Dodartii:* but even the British *S. binervosa* varies much in its leaves; some being much broader than others, although none resemble the figure in the Bot. Mag. Our plant has the scapes generally low and extremely variable in the mode of ramification; whereas Boissier says his plant has them thick, rigid, and often two feet high! I have seen no such plant from this country. In our herbaria, British specimens present sometimes the spikelets approximated and closely imbricated, forming short dense spikes; in others the flowers are more distant and the spikes slender: the former I have from Devonshire and Wales, the latter from Dover: but there are intermediate forms, and in all there is a tendency to produce sterile branches or branchlets: nor are the above differences accompanied with the other characteristics indicated by Boissier.

I may here state that *S. Limonium* has been said to grow on the Galway coast; but it is probable that this is rather *S. Bahusiensis* of Fries, (the *S. rariflora* of Drejer and Babington,) a plant which may, after all, be only the northern form of *S. Limonium*. It is found on various places of the Wigton and Kirkcudbright coasts. There has been lately an endless change of names among the Statices: even the well marked *S. reticulata* of England is now supposed not to be the Linnæan one, but the *S. Caspia* of Willdenow, found in England, France, and along the Mediterranean to the Caspian Sea. It is therefore no longer to be found in De Candolle's

Prodr. under the name we all know it by. As to *Statice bellidifolia, auriculæfolia,* and *spathulata,* there seems to be such an inextricable jumble, that we must either drop these names entirely, so as to prevent false ideas arising from the name, or we must conjoin many that have lately been disjoined. The volume of De Candolle containing Boissier's description of the Plumbagineæ has only been published a few months ago: I have just got it, and have as yet made only one hasty attempt to decipher my foreign species by it. I may, however, be permitted to remark, that the species seem to be founded on the philosophy of the modern schools, that plants differing in character, however trifling, if words can be found to express them, must be distinct species; instead of the older and more reasonable doctrine of botanists, that plants must have a natural specific difference before we can assign limits to their written characters.

Before leaving the lighthouse, (where we were requested to remain all night, if the four would nestle in a couple of beds,) we were shown the curious mechanism by which this modern Cyclops exhibits its solitary eye to the erratic mariner; and perhaps no mechanism raises up in the mind more lofty ideas than that exhibited by the lighthouses on our coasts, erected by Stevenson,—inferior to none in the world, superior to most.

We were then conducted to the Smuggler's Hole, and hoped to find some good things on the adjacent rocks, but were disappointed. Near the "hole" itself I found one solitary plant of the hairy variety of the common heath, *Calluna vulgaris.* On the shore about West Tarbet, we found *Crambe maritima,* or Sea Kale, in considerable quantity; a plant which, like the Samphires, becomes much more tractable by dipping it in boiling water.

From this point we pushed on, keeping the top of the rocks, instead of the coast, as the distance from Drumore was considerable, and we had much work yet before us. For this reason it is probable that we missed the *Euphorbia Portlandica,* said to grow along the coast. On coming near to Cardrain farm, (Mr. M'Culloch's,) we thought it advisable to call there, as we hoped to get him to conduct us to the little *Ononis reclinata,* almost the scarcest of Scotch plants. Unfortunately he was from home, and his lady could give us no assistance. We returned to the cliffs, but having only Dr. Balfour's notes in the Philosophical Society's Proceedings, (vol. i. 209,) to guide us, we were led to suppose that the plant grew on the top of the cliffs, along with the *Oxytropis Uralensis,* instead of at the bottom, on a gentle declivity near the sea. It must be confessed, too, that to find this plant in a good state, we were a month too late. *Geranium sanguineum* was abundant. *Alisma ranunculoides* and *Hypericum Elodes,* we did not see. The former I met with shortly before on the west side of the greater Cumbrae,—an island interesting for the endless profusion of *Anagallis tenella,*—and the latter we found afterwards nearer Portpatrick. On returning to Cardrain farm-house for our vascula which we had left there, we found by the side of a ditch, abundance of *Lepidium Smithii,* and a few specimens of *Stachys ambigua.* This latter plant is supposed by many to be a hybrid

between *S. sylvatica* and *palustris;* by others to be a slightly narrow-leaved form of *S. sylvatica;* that which we found differed only from *S. palustris* by the leaves decidedly, although shortly stalked, and more cordate at the base: that ours was the true plant, I have no doubt, since Mr. Bentham, the best authority in the world for the species of Labiatæ, has reduced it as a mere variety to *S. palustris;* a variety which, although he has called it *β hybrida*, appears to me to differ in no *permanent* degree, as every legitimate variety ought to do, from the type of the species. We also found the common state of *S. palustris* in the neighbourhood, but I did not observe any of the *S. sylvatica*, although very probably it is to be met with there likewise.

We returned to Drumore about seven o'clock, and thus concluded our first day's excursion.

Having seen our packages deposited in the Port Logan coach for Stranraer, which left Drumore at half-past seven next morning, we took the direction of Port Logan across the peninsula. The road, made chiefly for the use of the farm-houses, is extremely zig-zag. The distance was stated first to be three miles, then about three miles; as we got at least a mile on the way, Port Logan seemed to be farther off than when we started, and it was now about four miles. The true distance can scarcely be less than five or six miles; so that, although we did not loiter much by the way, we did not reach our destination till half-past nine. We found here *Peplis Portula*, and at Cowans, the willows alluded to by Dr. Balfour: they were *Salix aquatica, caprea, alba*, and *fusca;* the three first certainly cultivated: *Euonymus Europæus* may also grow here, but, like many others observed in the hedges, had obviously been planted. The necessary interlude of breakfast being performed, we directed our steps to the beach, where we observed *Calystegia Soldanella, Eryngium maritimum, Polygonum Roberti, Œnanthe Lachenalii, Atriplex laciniata*, and perhaps *A. rosea, Scirpus maritimus*, and a single plant of *Beta maritima*. It was my wish to examine *Scirpus maritimus* with attention, as in North America there are understood to be two very distinct varieties, or perhaps species, confounded under that name or its synonyme *S. macrostachyos* of Muhlenberg; and it is as yet doubtful which is our British one—if, indeed, we do not possess both. The one, the true *S. maritimus*, has the nut broadly obovate, lenticular, shining, and much longer than the hypogynous bristles, which are slender; the other, which more properly merits the name of *S. macrostachyos*, the *β fluviatilis* of Dr. Torrey, has the nut triangular, narrowed downward, dull, acuminated, and only as long as the hypogynous bristles, which are rigid: the first is found in salt marshes, or not far from the sea shore; the other, along the borders of lakes and rivers, always in fresh or only slightly brackish water. Those we found on the coast of Galloway I presume to be the true *S. maritimus* from the locality, but the fruit was so extremely young as to throw no certain light on the subject, or if in Europe the same differences of structure were connected with the difference of locality. I recommend it to the attention

of those who reside in Glasgow during the month of September, as fine specimens may be had between Bowling-bay and Erskine ferry, and elsewhere on the banks of the Clyde.

At Port Logan we visited the far-famed fish pond, where we see the inhabitant of the sea reduced to the same degree of tameness as a cat, by means of that mighty engine, hunger; an excellent illustration of that dominion which God promised to man, not only "over the fowl of the air and every living thing that moveth upon the earth," but also "over the fish of the sea." There was, indeed, something almost ludicrous in the familiarity shown by some of the larger ones, coming to the very edge of the pond, with their jaws expanded and half out of water, so as to be fed with a few limpets.

Passing for a short way through the grounds of Port Logan, we took the road to Port Gill, and met with the *Aster Tripolium*, *Scirpus maritimus*, *Helosciadium nodiflorum*, a variety of *Polygonum aviculare* considerably different from the ordinary one, and a small maritime form of *Catabrosa aquatica*. On some grassy slopes we found *Carlina vulgaris*. The shore being too much indented and rugged to permit of us following it, we again left it in search of the road to Portpatrick. In this portion of our journey we observed *Equisetum limosum*, (*E. Telmateia*) and in one or two places, *Isolepis Saviana*, a species which, although distinguishable from *I. setacea* by its nut being destitute of longitudinal furrows, has yet so precisely the same appearance, that in the absence of the fruit, it would have been impossible to decide which was the species. Besides *I. Saviana*, another species or form of this is found in some parts of England and Ireland: it is the *I. pygmæa* of foreign botanists, a name which is older than *I. Saviana*, although it is not very applicable to some of the elongated Irish and even Galloway specimens I possess. Under the name of *I. pymæa*, the plant has a very wide geographical distribution: as *I. Saviana*, it is peculiar to Europe. In the specimens collected in Galloway, the nut cannot be said to have any asperities or tubercles on its surface; it only appears rough, on account of minute and very close numerous impressed dots, like a thimble. After joining the road to Portpatrick, we met with *Radiola millegrana;* and in one spot on the east side of the road, *Tormentilla reptans*, a species which ought not to be confounded with *Potentilla reptans;* but if blended with any, it must be with *Tormentilla officinalis*, of which it has the ex-facie appearance of a very luxuriant variety. I am not aware if any attempt has been made to cultivate it, or compare it with *Tormentilla officinalis*, also cultivated: roots or seeds for that purpose may however be easily procured near Glasgow, close to Lambhill bridge across the canal, where I have seen it for two years in the month of July in great luxuriance.

Coming near Port Float, we descended to the beach, but found nothing worthy of notice, except *Ligusticum Scoticum;* and as much of the day was spent, we resolved on again seeking the road, and proceeding direct to Dunskey Castle, a distance of five or six miles. This part of our walk

was most unprofitable: indeed, throughout the whole peninsula called the Rhynns of Galloway, nothing of any consequence is found away from the shore; and the shore, particularly on the west side, is so bold and rocky, that it would consume as many days, as hours we devoted to it, to examine it properly; and were any one to attempt this, he ought to spend his chief time between the lighthouse and Clanyard Bay, and probably accommodation at night might be had at the lighthouse itself.

When we came in sight of Dunskey Castle, we left the road and passed through a moor. Here, about a quarter of a mile from the castle itself, we found *Hypericum Elodes* (*Elodes palustris* of Spach,) and *Potamogeton oblongus*. The Hypericum was in considerable abundance: the only spot along the Firth of Clyde where it has been found, is on the north side, opposite to Gourock, about two hundred yards to the west of Portkill in Dumbartonshire. As we were separated by a chasm from the castle, the party divided, two descending to the shore, the others keeping the high ground. The former found among the cliffs, *Rhodiola rosea* and *Helianthemum vulgare*. In the moat behind the castle were *Anagallis tenella*, (which, however, we had observed in several other places along the coast,) *Helosciadium inundatum* in a very fine state of fructification, and *Callitriche platycarpa*; with regard to the position of this last genus in a natural arrangement, there can, I think, now be no doubt, that in the youngest germen there is not the least trace of a margin or rim to denote an adherent perianth: the fruit is thus superior, and the affinity with Haloragese no longer tenable. It is difficult to compare it with any other known natural order; for it bears no close resemblance to any of the Euphorbiaceæ, proteus-like as that order is; and yet that is the group with which Lindley feels constrained to ally it.

Dunskey Castle is on the summit of the cliff, and close to its walls in front were magnificent specimens of *Arenaria rubra*, that form of it called by Linnæus β. *marina*, but not the *A. marina* of most English botanists, which is the *A. media* of Linnæus and De Candolle. I am not quite sure if I understand what Babington means by these species, as he dismisses the characters obtained from the ring of the seed, and relies on the round stems and angular rough seeds for *A. rubra*; and compressed stem and compressed nearly smooth seeds for *A. marina*. Now, in the Dunskey Castle plant, the root appeared perennial, as in the usual maritime forms of these species, the stem and seeds compressed, as in his *A. marina*, but the latter rough, as in his *A. rubra*. Then, if this be his *A. marina*, we have no character left but the compressed stem and compressed seeds, and these latter were by no means lenticular—a transverse section exhibits a triangle. Babington further assigns to his *A. rubra*, pointed leaves, while he says that in *A. marina* they are blunt: in the Dunskey plant they are decidedly mucronate. It is thus extremely difficult to pronounce what are species, and what varieties; nor can a mere European observer decide the question, as similar forms to our own occur in North America, *Buenos Ayres, Chili*, and the Cape of Good Hope; and it is useless

to draw up characters which only apply to European specimens, and perhaps not even to them universally. In fact, here, as in all other cases, we are not first to draw up a character, and decide on what is a species by its accordance therewith; but we must previously decide on what is a species, and then do our best to express its limits in words.

To the north of the castle were abundance of small species of *Daucus Carota*, probably the same that Dr. Balfour says resembles *D. maritimus*. The true *D. maritimus*, however, is a widely different plant, and is certainly not a native of Scotland. Between the castle and Portpatrick was abundance of a proteiform *Erythræa*, sometimes resembling the common state of *E. centaurium*, sometimes approaching *E. latifolia*. On the whole, however, I could not decide that there were more than one species in this locality, nor am I so much inclined to rely as Greisbach and others, on the proportion of the calyx to the tube of the corolla, so as to distinguish the *E. latifolia* from *E. centarium*: at first, in both, the whole bud is as short as the calyx; and when in fruit, the tube alone is considerably longer than the calyx: between these two limits there are numerous gradations; besides it is extremely difficult to say what the period of comparison adopted by Greisbach is, viz., the precise time of the corolla beginning to open; and if such be the only difference between the two, it is of little use, whether practically or theoretically, to attempt to distinguish them. At Portpatrick, we observed abundance of *Samolus Valerandi*, at the south end of the pier, near a large quarry; and on the pier itself, *Pyrethrum maritimum*, (surely a mere variety of *P. inodorum*,) *Coronopus Ruellii*, and an *Atriplex*, with oblong lanceolate quite entire leaves, but which may be only a form of *A. patula*. Along all the west coast we met with *Scilla verna*, or at least with its withered scapes.

From Portpatrick we returned to Stranraer in a car.

Galloway, or the Rhynns of Galloway, is altogether a most remarkable spot, whether for its form, or for its geological appearance, or its botanical productions. The name Rhynns, or Rins, is very ancient. Buchanan, in his History of Scotland, notices it under the Latinified name of Rinus, and says that the word means an edge or short point; and in this sense it may be traced, on the one hand, to the Greek word ῥὶς, and, on the other, to the Gaelic word Roinn, a point or promontory. Buchanan further states, that the name Galloway, or Gallovidia, is from the word Gallovid, which, in the old language of the country, means a cock. If this be correct, the Rhinns may be compared to its head and beak: but Buchanan may have merely conjectured this from the resemblance to the Latin word *Gallus*, for the Gaelic for that bird is not Gallovid, but Coileach: two preferable etymologies present themselves;- the one, the obsolete word Cailbhe, a mouth, and hence an estuary; the other, Gailbheach, stormy, from the frequent storms to which the coast is exposed. Buchanan elsewhere states, that the name was conferred on it by the Irish, (when ceded to them about the year 430,) in honour of *their own Galway*.

Having relinquished our intentions of going down the east side of Glenluce Bay, we returned next morning (Thursday) to Ayr by the steamer, and had an hour or two to examine the coast south of that town, before returning to Glasgow by the train. We met with *Juncus Gerardi*, or *cœnosus* of Bicheno, *Atriplex laciniata*, *Polygonum Roberti*, in abundance, perhaps more so than on the coast of Galloway; *Borago officinalis*, *Atriplex rosea*, but nothing worthy of interest.

The weather was excellent, except the single instance mentioned of a cold drizzle when at the lighthouse; and in this we were more fortunate than if we had been at Glasgow or Ayr, for in these places, on the Wednesday, there had been partial thunder and much rain. Indeed, while walking from Port Float to Dunskey Castle, we saw, from the clouds, that there was a thunder storm about the head of Glenluce Bay, and for some time we were not without fear that we should not escape, as the wind came from that quarter. It, however, dissipated before reaching us.

I have only to regret that none other of the party has agreed to give you this short account of our excursion, as I made no notes, and now speak entirely from memory. I have to thank Dr. Thomson for bringing some of the localities to my remembrance.

Mr. Crum then read the following paper:—

VIII.—*On a Peculiar Fibre of Cotton which is incapable of being Dyed.* By WALTER CRUM, Esq., F.R.S., VICE-PRESIDENT.

IN the month of May last, Mr. Thomson of Primrose received from Mr. Daniel Kœchlin of Mülhausen some specimens of a purple ground printed calico, each of them containing a portion of cotton which was white, although subjected to the same treatment by which the rest of the cloth, and even the threads which crossed the white one, was uniformly dyed. The white part of the thread was usually thicker than the rest, and little more than a quarter of an inch long. The whole fabric had been thoroughly bleached before printing, so that it contained no grease or other impurity that could resist the colouring matter.

White specks like these are not unknown or undreaded among the printers of calicoes in this country. Mr. Kœchlin mentions that the cotton of which they are formed is known by the name of *coton mort*, and here also it is called *dead cotton*. Mr. Kœchlin has been the first, I believe, to suggest that it may consist of unripe cotton, and that its fibre may be solid, wanting the hollow of the more perfect fibre. He adds, that if such should prove to be the case, its behaviour with colouring matters may affect materially the question of the mechanical or chemical nature of the union of cotton with its dye. Mr. Thomson did me the honour to transmit me the specimens for examination.

The ordinary cotton fibre, it will be remembered, is described by Mr. Thomson in the memoir where its form was first made known,* as a tube,

* Annals of Philosophy, for June, 1834. Lately reprinted in the Classical *Museum, No. 20, and in Liebig's* Annalen or January, 1849.

originally cylindrical, but which collapses in drying. It has then the appearance of two small tubes joined together, so that a transverse section of the filament resembles in some degree a figure of 8. Until full maturity, the cylinder is distended with water, in which bubbles of air are often distinguishable.

On placing a few of the fibres of the *coton mort* under the microscope, I found them to consist of very thin and remarkably transparent blades, some of which are marked or spotted, while others are so clear as to be almost invisible, except at the edges. These fibres are readily distinguished from those of ordinary cotton by their perfect flatness, without the vestige of a cavity, even at the sides, and by their uniform as well as great transparency. They are often broader, too, than the usual fibre, and they show numerous folds, both longitudinal and transverse, but they are never twisted into the cork-screw form of the ordinary fibre.

It occurred to me that cotton of this description might be detected among the wool as it is imported. I searched accordingly for any portions that had a different appearance from the rest, and having collected and examined them, I found one sort whose filaments had exactly the appearance, under the microscope, of the *coton mort* in the pattern of Mr. Kœchlin. It occurs in the form of a small matted tuft of a shining silky lustre, and usually contains in its centre the fragment of a seed, or perhaps an abortive seed. It consists of short fibres, having little tenacity. Specimens of it are found in abundance among the motes or hard portions, called droppings, rejected by the picking machine in the preparation for spinning. Small tufts of it, however, do occasionally pass the sifting process of the picking machine, and then, their fibres being too short to be teazed out in the carding engine, or drawn into threads in the subsequent operations of cotton spinning, remain as minute lumps or knots upon the threads of better wool.

Although the microscopic appearance of the fibre in question is that of a flat single blade, the cellular character of the tissue scarcely admits of such a formation. We must rather suppose that like the healthy unripe cotton fibre, it was originally an elongated cell or tube filled with liquid —that the seed around which it began to grow had died soon after its formation, while the fibres which clothed it were yet soft and pliable, and that the flattening, and perhaps growing together of the sides of the tube was occasioned by the pressure from the increasing crop of cotton attached to the numerous other seeds confined in the same pod.

To explain the bearing of this peculiar structure upon the question, whether cotton-wool and colouring matters form together a true chemical compound, or are held together by a merely mechanical power, I must quote a passage from a memoir on this subject which I read to the Philosophical Society six years ago, and refer to the memoir itself for additional illustrations.

"In many of the operations of dyeing and calico-printing, the mineral *basis of the colour* is applied to the cotton in a state of solution in a

volatile acid. This solution is allowed to dry upon the cloth, and in a short time the salt is decomposed, just as it would be, in similar circumstances, without the intervention of cotton. During the decomposition of the salt its acid escapes, and the metallic oxide adheres to the fibre so firmly as to resist the action of water applied to it with some violence. In this way does acetate of alumina act; and, nearly in the same manner, acetate of iron. The action here can only be mechanical on the part of the cotton, and the adherence, as I shall endeavour to show, confined to the interior of the tubes of which wools consist, or of the invisible passages which lead to it. The metallic oxide permeates these tubes in a state of solution, and it is only when its salt is there decomposed, and the oxide precipitated and reduced to an insoluble powder, that it is prevented from returning through the fine filter in which it is then enclosed.

"When the piece of cotton, which, in this view, consists of bags lined inside with a metallic oxide, is subsequently dyed with madder or log-wood, and becomes thereby red or black, the action is purely one of chemical attraction between the mineral in the cloth, and the organic matter in the dye vessel, which, together, form the red or black compound that results; and there is no peculiarity of a chemical nature from the mineral constituent being previously connected with the cotton."

To produce the purple dye of Mr. Kœchlin's pattern, the cloth has first to be impregnated with iron. For this purpose it is made to imbibe a weak solution of proto-acetate of iron, and afterwards dried. By exposure to the air for some days the salt is decomposed. Its acetic acid evaporates, and the oxide of iron, then become peroxide, remains in the fibre. The cloth is afterwards subjected to severe washings in hot and cold water, but its iron is not removed, and the question is, How is it retained in connection with the cotton? Mechanically, as I maintain, and probably in the interior of its hollow fibre, which it entered in a state of solution, and within which it was precipitated. Others, as I have already stated, are of opinion, after Bergman, that the combination is a chemical one; and so fully is that view carried out by my friend Professor Runge of Oranienburg, in his ingenious and excellent work on the chemistry of dyeing,* that he assumes coloured cottons to be combinations of what he calls cottonic acid with the various bases in definite, and even in multiple proportions. Thus a very pale shade of buff from oxide of iron, is called *percottonate of iron;* another is called *bicottonate of iron*, and still deeper shades *cottonate* and *basic cottonate of iron.*

But the new fibre, by the same treatment, is incapable of retaining the iron mordant, and yet both fibres have the same chemical composition, and the same ultimate structure. The only difference is that one is shaped into tubes or bags capable of holding all matters which are insoluble in water—that is, all bodies that can be caught upon a filter, while the other is possessed of no such inclosure.

I take this opportunity, in reply to a review of my first memoir on this

* *Farbenchemie.* 2 vols. Berlin, 1832 and 1845.

subject, by M. Persoz, in his remarkable work, "Traité de l'impression des Tissus," of explaining, that I attribute to an attraction of surface those cases of dyeing where pure cotton, by mere immersion, is enabled to decompose the solid matters in solution, and to withdraw them from the solvent. Such is the case with the solution of deoxidized indigo in lime—with the plumbite of lime—with the various salts of tin, and many other solutions. Cotton, as I have stated, acts in these cases like charcoal and other porous bodies, and I have seen no reason to confine the attraction in question to the internal surface of the cotton fibre.

But I have not ranked the aluminous mordant among the class of bodies so attracted, because cotton when immersed in a solution of acetate of alumina, has not the power of separating its basis. That solution must be applied to cotton and dried in it, and then the alumina only adheres, or loses the power of being washed away, in proportion as the acetic acid is removed by evaporation. I could see here no chemical decomposition effected by the cotton-wool, for the same salt may be decomposed by evaporation in a glass vessel. In this case I have represented the alumina as being held in the interior of the fibre, just as sand may be held in a bag whose interstices are too narrow to allow its particles to pass.

M. Persoz has remarked, however, that by evaporating a solution of acetate of alumina in a glass vessel, we do not so thoroughly decompose it as by drying the same substance upon calico. This I also have observed; and although I have been accustomed to ascribe the difference to the more extensive division and exposure of the salt upon cotton, I have no proof, and shall not deny that the presence of cotton at a particular stage of the evaporation may accelerate the decomposition of the salt, and that its fibres may thus attract a portion of alumina over their whole surface. If this modification of the view I had given be correct, the action of the *coton mort* proves at least that colouring matter adhering outside is not so permanent as that which is held within the fibre of the mature cotton.

Neither view gives any countenance to the chemical theory. Porous bodies are well known to attract, and even to decompose, without chemically combining with the substances they precipitate. Accordingly, none of the oxides are changed either in colour or in chemical character by their union with cotton. The hydrated oxide of copper, for example, precipitated upon calico, becomes carbonate, or arsenite when exposed to carbonic or arsenious acid. The protoxide of iron changes speedily in the air into the red sesquioxide, and that again may be converted into prussian blue, or into a black or purple lake—every new compound, if it only be insoluble, adhering firmly to the wool.

Mr. Crum also reported upon some new facts in the Chemistry of Digestion, and of Poisons.

The Society then adjourned till the next Session.

BELL AND BAIN, PRINTERS, GLASGOW.

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

FORTY-EIGHTH SESSION.

7th November, 1849.—*The* PRESIDENT *in the Chair.*

THE Forty-eighth Session of the Society was opened this evening, when Dr. Thomson, the President, read a paper on Distillation.

21st November, 1849.—*The* PRESIDENT *in the Chair.*

THE following were elected members of the Society, viz. :—Dr. Allen Thomson, Mr. James Cumming, Dr. Arthur Mitchell, Messrs. George Lyon, John Parker, James Napier, Robert Rankin, Andrew Renfrew, Charles Thomas, John Condie, Dr. Ebenezer Watson, Mr. William P. Smith.

Mr. Liddell, the Treasurer, gave in the following Abstract of his Accounts for Session 1848-49, together with Inventory of the Society's Property :—

1848.	DR.				
Nov. 11.—To	Cash in Union and Provident Banks at beginning of Session,	£32	19	11	
1849.					
Nov. 11. —	Interest on do.,.....	1	19	8	
					£34 19 7
—	30 Entries of New Members, at 21s.,.....	31	10	0	
—	14 Annual Payments from Original Members, at 5s.,.....	3	10	0	
—	215 Annual Payments, at 15s.,	161	5	0	
—	Arrears from one Member,.....	0	15	0	
					197 0 0
—	Balance due Treasurer,.....				1 17 10
					£233 17 5

1849.	Cr.						
Nov. 11.—By New Books and Binding,.....	£58	15	1				
— Printing Transactions, Circulars, &c.,.....	36	19	0				
— Alterations on Bookcases,.....	2	12	0				
				£98	6	1	
— Rent of Hall,.....	15	0	0				
— Coffee for Evening Meeting,.....	0	16	10				
— Fire Insurance,.....	2	16	0				
— Society's Officer, Clerk addressing Circulars, and Poundage Collecting Dues,.....	13	10	4				
— Postage, Delivering Letters, and Stationery,.....	8	15	1				
— Gas for Hall, &c.,.....	0	16	0				
				41	14	3	
— Subscription to Cavendish Society,.....				2	2	0	
— Cash in Union Bank,.....	90	0	0				
— Do. in Provident Bank,.....	1	15	1				
				91	15	1	
				£233	17	5	

Glasgow, 12*th November*, 1849.—We have examined the Treasurer's Account, and compared the same with the Vouchers, and find that there is in the Union Bank of Scotland the sum of Ninety Pounds sterling, and in the Provident Bank One Pound Fifteen Shillings and One Penny; and that the Treasurer is in advance One Pound Seventeen Shillings and Tenpence (say £1 17s. 10d.) sterling, thus leaving a net Cash Balance in favour of the Society of Eighty-nine Pounds Seventeen Shillings and Threepence. The Treasurer has also exhibited to us a Voucher which he holds for money lent to the Corporation of the City of Glasgow, from the proceeds of the Philosophical Society Exhibition in 1846, with the Interest thereon up to 15th May ult.,—the amount at said date of 15th May being Five Hundred and Five Pounds Sixteen Shillings and One Penny sterling.

THOMAS DAWSON.
WILLIAM COCKEY.

Note by the Treasurer.—14*th November*, 1849.—From the above Account it will be seen that the Income has exceeded the Expenditure in the amount of £56 17s. 4d. This has arisen chiefly from the increased number of Members admitted. But the Society is under liabilities to Booksellers about £55, which, when paid, will reduce the money stock to nearly the same amount as at commencement of last Session. The number of Members on the Roll at commencement of the Session was 222; new Members admitted, 30; making in all 252. Of these have died 8; resigned by letter, 5; and expunged from the roll of Resident Members for being in arrears, 8;—in all 21, to be deducted from the above-named number,—leaving 231 Members on the roll at this date. There are now 13 Members in arrear of dues for one year, the major part of whom have removed their residence out of Glasgow, but have not intimated this officially to the Secretary. Had this been done, conform to Law XI., and a desire expressed that their names should be retained on the list of Non-Resident Members, their privilege would have thereby been reserved of "resuming their position as Resident Members whenever they return to Glasgow, upon payment of the current year's subscription." By neglect of giving this intimation, the name is retained on the General Roll, and dues exacted till expiry of two years, at which period, if not paid, the name is then expunged from the list, and cannot *be restored without* a new election, and one guinea of entry-money paid.

In compliance with Law VI., the Treasurer now reports that the only property possessed by the Society consists of the following, viz. :—The Library (of which a new Catalogue is in the press). Portrait of President, in Gilt Frame. Marble Bust of President. President's Chair of Oak from Roof of Cathedral. Table in Hall, in two pieces. Writing Desk on do. Four Gas Lustres. Three Book-Presses. Small Black-Board. Stove in upper Library-room. Eight Benches with top-rails. Steps for Book-cases. Ballot-Box. Secretaries' Tin Box.—The Treasurer presents to the Society an Abstract of the Moneys paid for Books and Binding during the Session 1843-44 to this date, being six years:—

Session 1843–44,				£45	6 0
— 1844–45,				26	2 11
— 1845–46,				82	0 1
— 1846–47,				104	4 9
— 1847–48,				143	4 1
— 1848–49,	Paid,	£58 15 1			
— —	Not paid, about	55 0 0			
				113	15 1
				£514	12 11

On the motion of Mr. Crum, seconded by Mr. Hastie, it was resolved to place the name of Mr. John J. Griffin, of London, on the list of Honorary Members of the Society.

The Secretaries were instructed to make up a list of the Honorary Members, to be placed in future at the head of the printed list of the Society.

It was resolved to instruct the Treasurer to intimate to members falling into arrears with their annual subscription for two years, that their names will, agreeably to the laws of the Society, be erased from the roll unless the same is paid.

Mr. Hastie read a communication from certain native proprietors of the Calcutta Public Library, accompanying a presentation of Catalogue of the Library and last Annual Report. The letter solicited for the Library a copy of the Society's "Proceedings."

The Librarian reported on the state of the Library. In the course of the last two years considerable progress has been made towards the completion of the various series of periodicals, so as to render them valuable as sources of consultation. Previous to this period, the Library did not contain one complete journal. The Society is now annually in the receipt of 40 periodicals. Of 26 English periodicals, there are 18 complete from the commencement. Of the 9 French periodicals, only 1 is complete, viz., Quesneville's Revue Scientifique. And of the 4 German journals, only 1 is complete, viz., Liebig's Annalen der Chemie. During the last two years blanks have been filled up, and series completed in the periodical literature, to the extent of 146 volumes, at an expense of about £56, or 7s. 6d. a volume. To render the remaining journals worthy of a scientific library, 255 volumes require to be purchased, at an estimated cost of £97, at 7s. 6d. per volume. These it is proposed to supply gradually, a selection being made for the completion of one or two journals annually. The total number of volumes in the Library on the 1st of November, 1849, *was 1600.*

The Society proceeded to the annual election of Office-bearers, and the vote papers having been collected, Mr. Dawson and Mr. Cockey were appointed to examine the votes and report the result, and they retired for this purpose to another apartment. During their absence,

Mr. Charles Robb, civil engineer, read a paper explanatory of the principle of a new Portable Smith's Forge, Furnace, and Ventilating Apparatus, the invention of Mr. Chaplin, of Glasgow. Models of the machine were exhibited by Mr. Robb, showing its mode of operation. It is constructed wholly of iron, and may be folded up into the dimensions of three feet six inches, by two feet six inches, with eight inches of thickness. It is contained in an upright frame, and consists of an eccentric fan with three blades, which is set in motion by means of a crank handle. In the model exhibited, the ratio of the diameter of the driving wheel to that of the pulley on the fan spindle, was twenty to one. A hundred revolutions of the driving wheel per minute is a rate of motion that can with all ease be produced and sustained for a long period; and this would give a velocity of 2000 revolutions of the fan per minute, or a speed of 5750 feet at the circumference of the fan. To the upright frame, the receptacle for the fuel, which is simply an iron tray, mounted on wheels, for shifting, is attached by bolts, and to the end of this the cold water trough is affixed. A dead plate is also placed before the inner front of the frame, the design being thus to protect the blowing mechanism from the injurious influence of the heat. The main cause stated by Mr. Robb for the extraordinary efficiency of the machine, viewed in proportion to the slight power required for its impulsion, was the extreme lightness of the various parts of the fan, together with the accuracy and delicacy of their motion. In commenting upon its superiority to the common smith's forge, Mr. Robb claimed for it not only the qualifications of greater compactness and durability, (its titles to which are indisputable,) but also that of greater efficiency. As illustrative of its rate of heating, he referred to the experiments made with it in the blacksmith's shop at the Woolwich Dockyard, in the presence of the chief engineer and master shipwright. It was found then that a bar of iron, 1½ inch in diameter, could be brought to a welding heat in from three to four minutes. By a slight modification, involving merely the addition of a set of fire bars, through which the blast is directed, the instrument was also capable of being converted into an excellent shot-heater. In this respect, too, its capabilities were fully tested in the experiments previously mentioned. The application of the machine to the purpose of ventilating public buildings and ships was next illustrated. The application of the fan to this purpose is not new; but, as usually constructed, it requires a very considerable amount of power to impel it, a great proportion of which is absorbed in overcoming the inertia of its mass, and sustaining it in rapid motion. In the form given to the fan by Mr. Chaplin, however, the resistance arising from friction and inertia is reduced to a minimum, and the power applied is *almost wholly* employed in exhausting or pumping out the vitiated air,

and as is sometimes necessary in the case of ships, forcing pure air. Both these functions may be performed at pleasure by the machine under notice. The fan and driving apparatus are precisely similar to those previously explained, the only distinction being, that the blades are made considerably wider, and the forcing and exhausting apertures are situated on the same side. In the case of public buildings it is proposed to drive the ventilators by water-power, derived from the water-pipes which traverse the streets; in ships manual power will suffice; and in steam-boats the engine would be available.

Mr. John Wilson, Mr. Smith of Deanston, and Mr. Hart, made some remarks on the applicability of the machine to the ventilating of mines and other purposes.

Mr. Dawson and Mr. Cockey having finished the examination of the vote-papers, reported that the following had been elected Office-bearers for the current year, viz.:—

President.

DR. THOMAS THOMSON.

VICE-PRESIDENT,..WALTER CRUM. | LIBRARIAN,...R. D. THOMSON, M.D.
TREASURER,.....ANDREW LIDDELL. |

Secretaries.

ALEXANDER HASTIE, M.P. | WILLIAM KEDDIE.

Council.

A. ANDERSON, M.D.	WILLIAM GOURLIE.	PROF. WM. THOMSON.
A. BUCHANAN, M.D.	ALEX. HARVEY.	G.A.W. ARNOTT, LL.D.
JAMES BRYCE.	WILLIAM MURRAY.	JOHN WILSON.
THOMAS DAWSON.	JOHN STENHOUSE.	A. K. YOUNG, M.D.

5th December, 1849.—*The* PRESIDENT *in the Chair.*

MR. THOMAS CHAPMAN was elected a member.

The Librarian reported that Mr. R. Gardner had presented to the Society a copy of his Natural History of the County of Stafford. A vote of thanks was passed for the handsome present.

The Secretary reported that the Honorary Members of the Society are the following, viz.:—

Elected in 1826, Mr. Charles Chalmers, Bookseller, Edinburgh.
— 1826, Professor William Couper.
— 1827, Mr. Alexander Hastie.
— 1834, Professor Thomas Graham.
— 1849, Mr. John Joseph Griffin, London.

The following paper was read:—

IX.—*On some Remarkable Effects of Lightning observed in a Farm-house near Moniemail, Cupar-Fife.* Communicated by WM. THOMSON, ESQ., M.A., Professor of Natural Philosophy in the University of Glasgow.

THE following is an extract from a letter, addressed last autumn to me by Mr. Leitch, *minister* of Moniemail parish:—

"MONIEMAIL MANSE, CUPAR-FIFE,
26th August, 1849.

"* * We were visited on the 11th inst. with a violent thunder-storm, which did considerable damage to a farm-house in my immediate neighbourhood. I called shortly afterwards and brought away the wires and the paper which I enclose. * *

"I have some difficulty in accounting for the appearance of the wires. You will observe that they have been partially fused; and when I got them first they adhered closely to one another. You will find that the flat sides exactly fit. They were both attached to one crank, and ran parallel to one another. The question is, how were they attracted so powerfully as to be compressed together? * *

"You will observe that the paper is discoloured. This has been done, not by scorching, but by having some substance deposited on it. There was painted *wood* also discoloured, on which the stratum was much thicker. It could easily be rubbed off, when you saw the paint quite fresh beneath. * *

"The farmer showed me a probang which hung on a nail. The handle only was left. The rest, consisting of a twisted cane, had entirely disappeared. By minute examination I found a small fragment, which was not burnt, but broken off."

[The copper wires and the stained paper, enclosed with Mr. Leitch's letter, were laid before the Society.]

The remarkable effects of lightning, described by Mr. Leitch, are all extremely interesting. Those with reference to the copper wires are quite out of the common class of electrical phenomena; nothing of the kind having, so far as I am aware, been observed previously, either as resulting from natural discharges, or in experiments on electricity. It is not improbable that they are due to the electro-magnetic attraction which must have subsisted between the two wires during the discharge, it being a well-known fact that adjacent wires, with currents of electricity in similar directions along them, attract one another. It may certainly be doubted whether the inappreciably short time occupied by the electrical discharge could have been sufficient to allow the wires, after having been drawn into contact, to be pressed with sufficient force to make them adhere together, and to produce the remarkable impressions which they still retain. On the other hand, the electro-magnetic force must have been very considerable, since the currents in the wires were strong enough nearly to melt them, and since they appear to have been softened, if not partially fused; the flattening and remarkable impressions might readily have been produced by even a slight force subsisting after the wires came in contact.

The circumstances with reference to the probang, described by Mr. Leitch, afford a remarkable illustration of the well-known fact, that an electrical discharge, when effected through the substance of a non-*conducting (that is to* say, a *powerfully resisting*) solid, shatters it, with-

out producing any considerable elevation of its temperature; not leaving marks of combustion, if it be of an ordinary combustible material such as wood.

Dr. Robert Thomson, at my request, kindly undertook to examine the paper removed from the wall of the farm-house, and enclosed with his letter to me by Mr. Leitch; so as, if possible, by the application of chemical tests, to discover the staining substance deposited on its surface. Mr. Leitch, in his letter, had suggested that it would be worth while to try whether this case is an example of the deposition of sulphur which Fusinieri believed he had discovered in similar circumstances. Accordingly tests for sulphur were applied, but with entirely negative results. Stains presenting a similar appearance had been sometimes observed on paper in the neighbourhood of copper wires through which powerful discharges in experiments with the hydro-electric machine had been passed; and from this it was suggested that the staining substance might have come from the bell wires. Tests for copper were accordingly applied, and the results were most satisfactory. The front of the paper was scraped in different places, so as to remove some of the pigment in powder; and the powders from the stained, and from the not stained parts, were repeatedly examined. The presence of copper in the former was readily made manifest by the ordinary tests: in the latter no traces of copper could be discovered. The back of the paper presented a green tint, having been torn from a wall which has probably been painted with Scheele's green; and matter scraped away from any part of the back was found to contain copper. Since, however, the stains in front were manifestly superficial, the discolouration being entirely removed by scraping, and since there was no appearance whatever of staining at the back of the paper, nor of any effect of the electrical discharge, it was impossible to attribute the stains to copper produced from the Scheele's green on the wall below the paper. Dr. Thomson, therefore, considered the most probable explanation to be, that the stains of oxide of copper must have come from the bell-wire. To ascertain how far this explanation could be supported by the circumstances of the case, I wrote to Mr. Leitch asking him for further particulars, especially with reference to this point, and I received the following answer:—

"Moniemail, Cupar-Fife,
30*th Nov.*, 1849.

"* * I received your letter to-day, and immediately called at Hall-hill, in the parish of Colessie, the farm-house which had been struck by the lightning. * *

"I find that Dr. Thomson's suggestion is fully borne out by the facts. I at first thought that the bell-wire did not run along the line of discolouration, but I now find that such was the case. * *

[From a drawing and explanation which Mr. Leitch gives, it appears that the wire runs vertically along a corner of the room, from the floor, to about a yard from the ceiling, where it branches into two, connected with two cranks *near one another*, and close to the ceiling.]

"The efflorescence [the stains previously adverted to] was on each side of this perpendicular wire. In some places it extended more than a foot from the wire. The deposit seemed to vary in thickness according to the surface on which it was deposited. There was none on the plaster on the roof. It was thinnest upon the wall-paper, and thickest upon the wood facing of the door.* This last exhibited various colours. On the thickest part it appeared quite black; where there was only a slight film, it was green or yellow. * *

"I may mention that the thunder-storm was that of the 11th of August last. It passed over most of Scotland, and has rarely been surpassed for terrific grandeur,—at least beyond the tropics. It commenced about 9 o'clock, P.M., and in the course of an hour it seemed to die away altogether. The peals became very faint, and the intervals between the flashes and the reports very great, when all at once a terrific crashing peal was heard, which did the damage. The storm ceased with this peal.

"The electricity must have been conducted along the lead on the ridge of the house, and have diverged into three streams; one down through the roof, and the two others along the roof to the chimneys. One of these appears to have struck a large stone out from the chimney, and to have been conducted down the chimney to the kitchen, where it left traces upon the floor. It had been washed over before I saw it, but still the traces were visible on the Arbroath flags. The stains were of a lighter tint than the stone, and the general appearance was as if a pail of some light-coloured fluid had been dashed over the floor, so as to produce various distinct streams. All along the course of the discharge, and particularly in the neighbourhood of the bell-wires, there were small holes in the wall about an inch deep, like the marks that might be made by a finger in soft plaster.

"Most of the windows were shattered, and all the fragments of glass were on the outside. I suppose this must be accounted for by the expansion of the air within the house.

"The window-blind of the staircase, which was down at the time, was riddled, as if with small shot. The diameter of the space so riddled was about a foot. On minute examination I found that the holes were not such as could readily be made by a pointed instrument or a pellet. They were angular, the cloth being torn along both the warp and the woof.

"The house was shattered from top to bottom. Two of the serving maids received a positive shock, but soon recovered. A strong smell of what was supposed to be sulphur was perceived throughout the house, but particularly in the bed-room in which the effects I described before took place."

The following paper was also read:—

* These remarkable facts are probably connected with the conducting powers of the different surfaces. The plaster on the roof is not so good a conductor as the wall-paper, with its pigments; and the painted wood is probably a better conductor *than either.*—*W. T.*

X.—*On Sanitary Reform and the Use of Sewage Water of Towns.* By JAMES SMITH, Esq., late of Deanston.

THE author began by observing that the causes of disease are to a great extent within our own power, and can be removed or diminished. There was much less sickness when the population was smaller and less crowded together; and it is found that the more populous a town becomes, the greater is the increase of endemic disease. Much of the disease in every country is caused by poverty and the want of domestic comfort, and above all, by irregular habits. The latter cause is to be removed by moral means; the former, so far as it can be removed, by physical means. The most prominent cause of disease is the accumulation of the various putrifying matters generated by the community. He described the pernicious effects arising from defective sewage, and especially the decomposition of animal and vegetable matter in cess-pools. The removal of these substances without allowing any portion of them to escape would be the perfection of sewerage. This requires an unlimited supply of water available at all times under pressure in every apartment where matter is generated, to provide proper orifices for receiving it into air-tight sewers; such orifices to be thoroughly water-trapped, to prevent the possibility of the escape from the sewers of any smell or gas whatever into the apartment, which can be effectually done at small expense. The first thing to be done, therefore, in order to the removal of these noxious matters, is to provide an abundant supply of water. Then the question arose, are these decomposing substances useless, and to be thrown into the nearest river? or can they be made useful to the community by which they are generated? He proceeded to answer this question by showing how it could be applied to the purposes of agriculture, so that it could not only be got rid of altogether, but rendered, at the same time, a source of profit to the community. Various methods had been proposed for this purpose; some proposing that the matters of suspension should be allowed to deposit in ponds, to be afterwards dried, and used as a light portable manure; the watery part being allowed to flow into the rivers as heretofore; not adverting to the fact, that the watery part contains the greatest amount of enriching matter in solution. Others have proposed to precipitate the matter in solution, as far as it can be done, by cream of lime or some such cheap agent, by which there would still be a sacrifice of the ammonia and alkaline constituents. Such treatment implies a considerable extent of space for the necessary apparatus, a considerable quantity of material for precipitation and desiccation, besides a great amount of expensive manipulation, whilst it has been ascertained that the application of the materials in their state of suspension and solution, as in the recent sewage water, is much more efficacious in promoting the growth of plants, than when extracted, dried, and otherwise prepared. But if it shall at any time be found that the manufacture of a portion of the manure in the dry state *shall be desirable,* manufactories can be established through the

country on any of the main lines of pipe, apart from the town and the dense population. The removal of the sewage water in its original fluid condition by pumping, and its conveyance in pipes to the spot where it is to be applied, obviously afford a simple and efficient mode of dealing with it. It has been ascertained by careful calculation that sewage water can in this manner be taken up and conveyed a distance of ten miles, and be thrown upon the ground in most equal distribution, at a cost of 3d. per ton; containing all the elements, unchanged and undiminished, and be presented to the great chemist Nature, to be dealt with by the never-ceasing and costless labourers of the vegetable kingdom. The pumping, conveyance, and distribution had been tested at several places on a sufficient scale to demonstrate its perfect practicability and efficacy. The first experiment of the pumping, conveyance in pipes, and distribution by hose, was made at Clitheroe, in Lancashire, under Mr. Smith's direction, by Mr. Henry Thomson of the extensive print-works there. The liquid consisted of the sewage of a village of the ordinary character, with that from the works, which contained, of course, a greater amount of soap suds and alkaline matter; this was thrown into a tank containing the drainage from a farm-yard. The pumping, conveyance, and distribution by the hose, answered admirably. The experiment was made on some old meadow land. This liquid was applied to a meadow at the rate of about eight tons per statute acre, whilst farm manure at the rate of fifteen tons per acre was applied to a corresponding extent. The grass from the liquid manure grew considerably more luxuriant than that from the farm-yard manure; but, unfortunately, the relative weights produced were not ascertained on cutting. The next experiment was made on a farm on the estate of Possil, near Glasgow, by Mr. Robert Harvey, of the Port-Dundas Distillery, who, in the most enterprising and spirited manner, at Mr. Smith's suggestion, laid down pipes for conveying liquid manure over his whole farm, consisting of upwards of three hundred acres, and had the liquid manure from a dairy of upwards of five hundred cows pumped to an altitude of more than seventy feet. The distribution of liquid manure has been carried on on this farm for more than four years, very little solid manure being used except for comparative purposes. The distribution is chiefly on what may be termed the low pressure system, as instead of being jetted with force to form an artificial shower, it is simply allowed to be discharged upon the surface (by tin-plate pipes of about an inch and half in diameter, and four feet in length, fitting into each other with slip joints, and these can be led to discharge the liquid at any point within their range, which can be extended to a length of two or three hundred yards, if necessary). In this manner, one man can distribute the manure over from one to two acres per day. The manure is thus applied to pasture land, to grass for cutting, for house-feeding, and for making hay. It is applied also to stubble land, and to fallow, and has uniformly raised magnificent crops of grass, potatoes, turnips, wheat, beans, barley, and *oats, on land not of* the best quality. These men are employed during

the whole year in distributing the manure, and go on in wet weather as well as in dry. Mr. Harvey has lately extended his system of pipes, and has erected a twelve-horse engine, which is more than master of the work. The Sewage Manure Company of London, which obtained Acts in 1846 and 1847, for taking a portion of the sewage water of Westminster, has, after much untoward obstruction and delay, got to work with a thirty-horse engine, and is now distributing the liquid in several of the market gardens at Fulham, and upon meadow land in the neighbourhood, with most satisfactory results. The Company receives £3 10s. for the season's watering of the garden ground, and £2 for that of the meadow land. The results in growing lettuce have been very extraordinary, a market gardener admitting that he had sold the lettuce from an acre of land, so watered, fourteen days earlier than that from some land which had not been watered, and that the pecuniary result had been £25 per acre more. The operations of the Company are going forward, and in another season the value of liquid sewage manure will be fully demonstrated. The water at present applied by the London Company is very much diluted, and has very little smell, and being immediately absorbed by the ground, all offence is avoided. It was at first thought by the engineer of the Company that it would be necessary to employ their own servants to make the distribution; but it has been found in practice that the men, women, and boys who are usually employed about gardens are quite competent to the work under the direction of the master gardener, so that the whole matter is, in the meantime, left in his hands, to use the liquid as he thinks most fitting. It has been objected by some that the distribution of the sewage water would generate miasma all over the country, to which it may be replied that the matter taken from the sewers being in a fresh condition, and before it has had time to pass into any extensive decomposition, and being in itself much diluted with water to facilitate its conveyance and distribution, and being thrown over an extensive absorbing surface, with a great area of atmosphere, any poisonous matter that may emanate from it will be so diluted that it cannot affect the health of man or beast. It is impossible, with present information, to determine what may ultimately be the profit derivable to any community from this source; but taking what data we have from scientific inquiries, as well as that from the practical experience which has been worked out, it does not seem extravagant to anticipate a free yearly income of one pound for each individual of the community. But to render the estimate safe, in the first instance it may be made at ten shillings, which would afford to the city of Glasgow an income of at least a hundred and fifty thousand pounds, which would put it in the power of the public authorities to root out by degrees all the narrow and unwholesome lanes and the wretched dwellings, which are a disgrace to the present age, and to carry on continuously the progressive improvement of the city. It has been suggested by many that customers will not be found for the manure in this condition to so great an extent; but it must be *obvious to* every intelligent agriculturist who takes the

trouble to make a survey of the surrounding country, and it will come home to the experience of every farmer, that there is every where a great want of manure to produce the fullest effect. If manure can be furnished in this manner at half the cost of ordinary manure, and be laid on the ground when the farmer wishes it, without carting over his land, with very little trouble to himself, and with results beyond the average of ordinary crops, there can be no doubt of finding customers every where. The crops of the farmer who uses this manure will excite the jealousy of his neighbours, which will lead them, I would almost say force them, to follow his example.

The question of the disposal of the sewage water in the liquid form, for agricultural purposes, being determined, the engineering of the system of sewers will be greatly simplified and rendered independent of tidal influence, as wells can be put down at points most convenient for the drainage of specific areas, and for the transmission of the liquid by the nearest route to the country. The sewerage should consist of a double system of air-tight tubular sewers, the one to receive the sewage water, the other to receive the rain water falling upon the streets and houses. All the inlets being securely trapped, so as to prevent the escape of any gas from the sewers, whatever gas may be generated must find its way to the general outlet, when it can be passed through the furnace of the pumping engine, and be thereby deprived of its noxious qualities, and be thrown into the air at a height above the streets and dwellings.

In order to test the usefulness of the manure in the liquid form, Mr. Smith said he had made extensive inquiries for several years. A number of experiments had also been made with the view of testing the practicability of dealing with it in the liquid form, and ascertaining the cost at which it could be pumped into pipes, and thus conveyed into the country. It was ascertained that sewage water can be conveyed, by pumping, ten miles, and delivered on the ground for 3d. a ton, with a moderate profit; whereas, to cart a ton for ten miles costs 5s. The liquid is not only conveyed at this charge, but distributed. The expense of distributing liquid manure by cart is considerable, and in the solid form it is more expensive still. Mr. Smith next remarked that the sewage water of large towns contains all the elements of the food used by the inhabitants and by animals. These elements exist in the state of mechanical mixture, of suspension, and of solution. A large dilution of these matters with water not only deprives them of smell, but fits them better for being applied to the land. The suspended matter moves easily in properly constructed sewers, and can be conveyed with facility through pipes. The sewage matter applied in the liquid form is much less liable to be washed away by rain, than when applied in the solid state; and besides, both chemical investigation and the results of experience combine to show that solid manure is less valuable than manure in a state of suspension or solution. In the Meadows at Edinburgh, which are irrigated by sewage water, it is *found that the matter* in suspension has a tendency to lodge at the roots

of the grass, causing decay; while the fluid manure which ultimately remains, after flowing through several meadows, produces the healthiest and best grass. Some of these meadows are let for £30 to £50 for the cutting of the grass. Six different specimens of water from the meadows, of a gallon each, yielded—

1. Water taken up immediately on its leaving the sewer, 244 grains of solid matter, and 82 grains in solution.

2. Taken as it flowed from subsidence pond, 52 grains of solid matter, and 87 grains in solution.

3. Taken after having flowed over one plat, 31 grains of solid matter, and 89 grains in solution.

4. Taken after having flowed over several plats, 15 grains of solid matter, and 82.7 grains in solution.

5. Taken still farther on, 2½ grains of solid matter, and 67.2 grains in solution.

6. Taken at the sea when passing away, 2½ grains of solid matter, and 72.9 grains in solution.

The meadows farthest from the source of the sewage water consist of poor sandy land, yet they produce better grass, in consequence of the water being deprived in its progress of its grosser matter held in suspension. Mr. Smith observed that sewage water contains a larger proportion and variety of nutritious matter than even guano.

Mr. Smith illustrated by diagrams the method of sewerage recommended by him, showing the importance of having a sufficient and uniform fall in the sewerage, and of using small air-tight pipes, instead of the large ones commonly employed.

19th December, 1849.—*The* PRESIDENT *in the Chair.*

THE following were elected members of the Society:—Messrs. John Paterson Brown, Hugh Wilson, Moses Provan, Robert Walker, William Mirrlees, jun.

On the motion of Mr. Gourlie, seconded by Dr. Walker Arnott, it was unanimously agreed to elect Dr. Balfour, Professor of Botany in the University of Edinburgh, an Honorary Member, in consideration of his eminent services to the Society during his residence in Glasgow.

Mr. Crum proposed, on the recommendation of the Council, that the next meeting of the Society should be held on the 9th proximo, and be a conversational meeting, which was agreed to. The following paper was agreed to:—

XI.—*On Reinsch's Process for the detection of Arsenic. By* HARRY RAINY, M.D., *Professor of Forensic Medicine in the University of Glasgow.*

This process consists in boiling the suspected fluid with about $\frac{1}{10}$ of its bulk of muriatic acid along with copper. The arsenic is deposited on the copper in the form of a steel-grey film.

It is generally supposed that this process is equally applicable to all the compounds of arsenic soluble in dilute muriatic acid; and that in all circumstances it detects the presence of the metal, with a delicacy more than sufficient for every practical purpose.

Soon after the publication of Reinsch's method, I made various experiments, with the view of determining the limits within which its indications might be relied on. The result was unsatisfactory; for while, in some cases, it appeared to be fully as delicate as the method of Marsh, in other cases I failed to obtain the metallic deposit where the arsenic was present in a much higher proportion. Similar observations have been made by others; for it is stated by Fresenius and Von Babo that, "the presence of nitrates and various salts of mercury, and other metals, render the separation of arsenic by copper difficult or even impossible." It seems also to be a general opinion, that when the proportion of arsenic is extremely minute, the process of Marsh is decidedly preferable.

It is obviously important that the cause of such discrepancies should be investigated, as the great simplicity and rapidity of Reinsch's process render it peculiarly suitable for medico-legal investigations, and give it a decided superiority over every other, if it can be conducted in a manner that will ensure equal delicacy.

The following experiments were made with the view of ascertaining the cause of these discrepancies, and, if possible, the means of preventing them. The copper was used in the form of very thin foil, which was easily cleaned and polished, so as readily to show any change of colour; the fluid usually contained one *tenth* part, by measure, of muriatic acid of the ordinary strength, except when the object was to ascertain the effects of varying this proportion; and in order to prevent any diminution of the fluid, or any change in its strength during the boiling, a condenser, containing cold water, was placed closely over the mouth of the vessel i which the process was carried on.

1. My first object was to ascertain the *extent of copper surface* that can, in the most favourable circumstances, be distinctly coated by a given quantity of arsenic. The results were very uniform. One thousandth of a grain of arsenious acid gave a full steel colour to *one* square inch of copper surface. When *two* square inches of copper are used with the same quantity of arsenious acid, the effect is still distinct; but the deposit is, in these circumstances, so thin, that there is a tinge of yellow, apparently from the copper shining through, or not being uniformly coated. *Two* square inches is the *utmost* extent of copper surface that can be distinctly coated by *one thousandth* of a grain of arsenious acid; and it can be proved that, in these circumstances, the thickness of the film of deposited metal does not exceed $\frac{1}{4.000.000}$ (one-four millionth) of an inch.

It follows from this result, that if the extent of copper surface be too great relatively to the arsenic present, no distinct deposit will be obtained; thus, a fluid containing one thousandth of a grain of arsenic, with *three square inches of copper* surface, might give a tarnish, but no distinct coating.

2. The effect of *dilution* was next examined. When the fluid was to the arsenic as *one million* to one, the deposit was distinctly formed in fifteen to twenty minutes. Thus, one thousandth of a grain of arsenious acid in one thousand grains of fluid, and consequently constituting one millionth part, gave a distinct coating to one square inch of copper surface in twenty minutes. The same quantity of arsenious acid in two thousand grains of fluid, also gave a deposit on the copper; but it was less distinct, and required a longer time. It appears then, that with a dilution of one million times, the effect is distinct and prompt, and when the dilution is carried to two million times, it is indistinct and tedious. A dilution of two million times appears to constitute the practical limit in Reinsch's process. By continued boiling it is easy, of course, to concentrate the fluid, so as to bring the dilution within these limits, if arsenic be present in any proportion, however small; for there appears to be no loss of arsenic by evaporation during the boiling.

3. The proportion of muriatic acid in the solution has a considerable influence on the *rapidity* of the deposition and even on its *production*, when the arsenic is in very minute quantity. Thus, if the arsenic is less than one millionth, the process is very slow in a fluid containing one tenth muriatic acid of the ordinary strength; but when it amounts to one seventh or one sixth, the deposition is much accelerated. And in solutions in which the quantity of arsenic is so small, that with the ordinary proportion of acid no deposit is obtained, the copper becomes distinctly coated if the proportion of muriatic acid is doubled.

4. From these observations it would follow that the rapidity with which copper acquires a distinct arsenical coating, is directly as the proportion of arsenious acid and also of muriatic acid in the solution, and inversely as the extent of the copper surface.

5. As copper receives a coating of a similar colour from other metals, from sulphur and sulphuretted compounds, the mere formation of such a deposit cannot be considered a conclusive proof of the presence of arsenic. It is merely a convenient method of *separating* the suspected substance, in order that it may be subjected to the appropriate tests. The most satisfactory of these tests are, the formation of a white crystalline sublimate by heating the coated copper—the solution of this sublimate in water, and its conversion, by the appropriate reagents, into arsenite of silver—orpiment—and arseniate of silver, all of which are very easily recognised by the peculiarities of their colour and other properties. In estimating the value of Reinsch's process, it is therefore necessary to ascertain, not only the smallest quantity, and the utmost dilution under which it can be separated and distinctly exhibited on copper, but also the smallest quantity which, when so separated, can be satisfactorily subjected to the conclusive tests.

In repeated experiments, I found that one thousandth of a grain of arsenious acid in one million times its weight of fluid, could be separated as a distinct *deposit on copper*. The copper thus coated, when heated

gently in a small tube, yielded a slight but distinct sublimate, most obvious on a black ground, and which, with a magnifying power of ten to twenty diameters, was found to consist of crystals with triangular facettes, and which, when dissolved in water, yielded orpiment and the red arseniate of silver, when treated with the appropriate reagents.

This I believe to be as great a degree of delicacy as has actually been obtained by the more tedious and troublesome process of Marsh; and is more than sufficient for every practical purpose.

6. When investigating the delicacy of Reinsch's process, I prepared quantities of very dilute solutions of arsenious acid, varying in strength from $\frac{1}{10.000}$ to $\frac{1}{100.000}$, and kept these solutions in readiness for the experiments which I had planned. When first tried with copper and muriatic acid, they gave results entirely conformable to those already stated. A portion of any of these solutions, containing one thousandth of a grain of arsenious acid, when diluted, so that the fluid amounted to a million times the weight of the arsenic, gave a distinct and rapid deposit on the copper; but afterwards I could obtain no deposit from larger quantities of arsenious acid, though in a more concentrated state. As an example—a portion of solution containing $\frac{1}{2000}$ of a grain of arsenious acid in sixty grains of water, and consequently with a dilution of one in twelve thousand, gave *no deposit whatever* when boiled in the ordinary way with copper and muriatic acid for upwards of fifteen minutes. In this case the surface of the copper was only $\frac{1}{8}$ of a square inch, and therefore could not interfere with the result by its too great extent.

I was perplexed with the apparent inconsistency of these results with those previously detailed, and began to suspect that I had been misled in my first estimate of the extreme delicacy of Reinsch's process. But after repeated trials I found the difference to depend on the length of time that the solution is kept. *Very dilute solutions of arsenious acid become gradually less and less sensitive to Reinsch's process*, so that after several weeks no deposit can be obtained on copper from solutions containing arsenic in the proportion of *one* in *fifty thousand*, or even one in twenty thousand. I have recently found, however, that the addition of a small quantity of any animal matter, such as milk, effectually prevents this change.

7. I was thus led to examine whether these dilute solutions underwent any appreciable change in their chemical properties, and found that with nitrate of silver they gave a white cloud—when concentrated by evaporation to a small bulk, the residual fluid strongly reddened litmus, and when evaporated to dryness, left a white stain, which did not sublime at a low red heat. This stain redissolved in a few drops of water, forming a solution which still strongly reddened litmus, and which, on the addition of a strong solution of nitrate of silver, gave a brick red precipitate.

These experiments clearly indicate the conversion of *arsenious* acid into *arsenic* acid.

8. In all watery solutions of arsenious acid this change appears to *take place to a certain* extent, if the solution is kept for a considerable



time. I have uniformly found arsenic acid in such solutions, if concentrated to a small bulk by evaporation. The residual liquor reddens litmus and gives a brick-red precipitate with nitrate of silver. But this conversion of arsenious into arsenic acid is *restricted within very narrow limits*, as the arsenic acid increases the change goes on more slowly; and when the arsenic acid amounts to one part in ten or fifteen thousand, *no further conversion takes place*. Hence, the whole, or nearly the whole of the arsenious acid may be converted into arsenic acid in a very *dilute solution*, while in a *concentrated* solution, though the same absolute quantity is so converted, it will bear a trifling proportion to the arsenious acid which still remains unchanged. I have recently ascertained that solutions of arsenious acid, containing animal matter, do not undergo this change, but remain, after an interval of several months, as sensitive as ever to Reinsch's process.

9. I cannot assign the cause of this change with absolute certainty, but there are strong reasons for believing that it depends on some principle communicated from the air; for it occurs more readily if the solution is exposed to the air in an open vessel, or kept in a vessel only partially filled and often shaken. It is natural to suppose that it is the oxygen of the air which, in these circumstances, unites directly with the arsenious acid. This opinion is in accordance with the partial conversion of sulphurous acid into sulphuric acid, when its aqueous solution is in contact with atmospheric air. Still I can adduce no proof of the correctness of this supposition, and it is conceivable that chlorine, or some nitrous compound in the atmosphere may be the real agent.

10. Supposing, however, that *arsenious* acid, in very dilute solutions, is gradually changed into *arsenic* acid, will this account for the fact, that such solutions gradually become less sensitive to Reinsch's test? This leads us to examine how solutions of *arsenic* acid, prepared in the usual way, are affected by that test. Is arsenic deposited on copper as readily in solutions containing *arsenic acid*, as in solutions containing arsenious acid? I believe it is generally supposed that there is no material difference. Mr. Taylor, in his valuable work on Toxicology, represents the process as no less applicable to arsenic acid and its combinations than to arsenious acid.

I made several experiments on this subject several years ago, and having operated on very dilute solutions, such as one part of arsenic acid in ten thousand parts of water, I was led to conclude, prematurely, that arsenic *cannot* be detected by Reinsch's process, when it is in the state of arsenic acid. This inference was erroneous; for the process succeeds partially when tried with moderately dilute solutions; still the difference between arsenious and arsenic acid is very great, for a solution of one part of arsenic acid in *fifteen hundred* parts of fluid, containing one tenth of ordinary muriatic acid, gives *no* deposit when boiled with copper, while one part of arsenious acid, as already stated, gives a distinct deposit when diffused in *one million* parts of a similar fluid. From

numerous comparative experiments, it follows that arsenious acid is a thousand times more sensitive to Reinsch's process than *arsenic* acid; or, in other words, in order to give a similar deposit, arsenic acid must be present in a thousand times greater quantity.

An increase in the proportion of the muriatic acid promotes the deposition, so that indications of the presence of arsenic may be obtained with weaker solutions. Thus, a solution of $\frac{1}{1000}$ gave no deposit, when the muriatic acid formed $\frac{1}{10}$, a slight tarnish when the acid was $\frac{1}{8}$, and a distinct steel-grey deposit when the acid was $\frac{1}{5}$.

In solutions containing $\frac{1}{10.000}$ of arsenic acid, I obtained no deposit even when the mixture contained $\frac{1}{5}$ of the muriatic acid of ordinary strength.

11. These statements will serve to show that Reinsch's process is not applicable to the detection of arsenic when it is in the state of arsenic acid. They also explain, in a satisfactory manner, how the process becomes impaired in its delicacy when applied to dilute solutions of arsenious acid, which, by keeping, is gradually changed into *arsenic* acid.

12. The injurious effects of nitrates and other compounds, such as the persalts of mercury, is also explained by these facts; for when such substances are present along with muriatic acid, they readily convert the arsenious acid into *arsenic* acid, and thus render it much less sensitive to Reinsch's process.

13. In conformity with these views, it might be expected that if arsenic acid were reduced to the state of arsenious acid, it would be brought into a suitable condition for the application of Reinsch's process. This can be accomplished by sulphurous acid. If a current of sulphurous acid gas is passed through the mixture, the arsenic acid is changed into arsenious acid, and the process resumes all its original delicacy, as I have ascertained by numerous experiments both on dilute solutions of arsenic acid, and dilute solutions of arsenious acid altered by keeping.

14. The mixture which is to be examined should first be boiled for a few minutes with the proper porportion of muriatic acid. It should then be allowed to cool, and a current of sulphurous acid gas should be passed through it till it is thoroughly saturated. This is most conveniently done by heating a mixture of sulphuric acid and charcoal in a flask furnished with a suitable tube for conducting the gas. Carbonic acid is produced at the same time, and escapes along with the sulphurous acid; but it does not, in any respect, interfere with the process.

It is indispensably necessary, however, that the sulphurous acid should be kept in contact with the arsenic acid for some time. The reduction of the arsenic acid into arsenious acid is a gradual process. I have sometimes found two hours necessary for its completion. When a sufficient time is elapsed, the superfluous sulphurous acid should be boiled off, till all smell of sulphurous gas is gone, the copper may then be introduced, and the process completed in the usual way.

15. Deposits bearing a considerable resemblance to the arsenical film

are produced by boiling copper in solutions containing free sulphur, sulphuretted hydrogen, and the combinations of sulphuretted hydrogen with bases, sulphuret of copper being thus formed as a thin film on the metal. The presence of muriatic acid is in no respect necessary for this reaction.

I thought it possible that sulphurous acid might act in a similar manner, and give rise to a coating of sulphuret of copper. On trial, I found this supposition incorrect; for the copper undergoes no change in its colour or lustre when heated in a watery solution of sulphurous acid.

If, however, muriatic acid and sulphurous acid are present together in a solution, the copper is speedily changed. It assumes a steel colour of a blueish tinge, dependent on the decomposition of the sulphurous acid and the consequent formation of a thin film of sulphuret of copper. In this reaction we have $SO_2 + 2\,HCl. + 3\,Cu = SCu + 2\,HO + 2\,Cl\,Cu$ —1 atom sulphurous acid + 2 atoms hydrochloric acid + 3 atoms copper, yield 1 atom sulphuret of copper, 2 atoms water + 2 atoms protochloride of copper.

This bears a close analogy to the action of hydrochloric acid—arsenious acid and copper in Reinsch's process in which $3\,HCl + 3\,Cu + AsO_3 = \overline{3Cl\,Cu} + 3\,HO + As$, or 3 atoms hydrochloric acid + 3 atoms copper + 1 atom arsenious acid yield 3 atoms protochloride of copper + 3 atoms water + 1 atom metallic arsenic. It is also interesting to observe, that while the combined action of copper and muriatic acid can decompose arsenious acid and sulphurous acid, on the more stable compounds, arsenic acid and sulphuric acid, they act on the one very feebly and on the other not at all.

16. I have been led into these observations of sulphurous acid, in order to obviate an objection which might be made to its employment, in bringing the arsenical solution into a fit state for Reinsch's process. It may be objected that the presence of sulphurous acid may itself cause a deposit on the copper, and thus mislead the experimenter. This difficulty, however, is obviated at once by boiling off the superfluous sulphurous acid before the copper is introduced. This will remove every ambiguity. But besides this, the subsequent testing, which is indispensable in every case, will afford complete security against error.

17. In a former part of the paper I mentioned that dilute solutions of arsenious acid, when long kept, give a white cloudiness with sol. of nitrate of silver, whereas it is well known that nitrate of silver gives no precipitate whatever in solutions of perfectly pure arsenious acid. The occasional production of this cloudiness in solutions of arsenious acid has frequently been noticed. I am not aware that any attempt has been made to account for it. I have satisfied myself that it arises from the presence of arsenic acid, produced in these solutions, as I have already explained, by exposure to the air. The arseniate of silver, in its ordinary form, no doubt is of a deep brown or red; but the colour varies greatly with the state of *dilution of the fluid*—when concentrated, it is of a reddish brown

—when more dilute, brick-red—when still more dilute, greyish, and when extremely dilute, as $\frac{1}{10.000}$, a whitish cloud, exactly similar to that which is observed in solutions of arsenious acid after they are kept for some time.

9th January, 1850.—MR. CRUM *in the Chair.*

THE Society held a conversational meeting.

The following were elected members:—Messrs. Alexander Mitchell, Gilbert Lang, Alexander Reid, James Ritchie, Andrew Risk, Laurence Clark, John Cuthbertson.

23d January, 1850.—*The* VICE-PRESIDENT *in the Chair.*

THE following were elected members:—Dr. James Steven, Messrs. James Robert Napier, Charles Thorburn, James M'Kenna, James Graham, Thomas Ferguson, John Burnett, George M'Callum.

Letters were read from the Royal Society of London, Royal Institution, Liverpool Literary and Philosophical Society, acknowledging receipt of Vol. 3d, Part 1st of Proceedings of the Society.

The Librarian intimated that Robert Blackie, Esq., had presented to the Philosophical Society's Library a copy of the Imperial Dictionary, in 2 vols., recently published by the Messrs. Blackie, and moved the thanks of the Society, which were unanimously given.

Mr. Stein exhibited and described a machine invented by him for checking the charge of duty upon spirits. A communication was made by Dr. R. D. Thomson, of a simple and continuous method of washing filters, by Mr. Eustace Cary Summers, which has been published in the Philosophical Magazine, vol. 35, p. 96.

6th February, 1850.—*The* PRESIDENT *in the Chair.*

THE following were elected members:—Messrs. David Wilson, George Simpson, James Manson, John Blackie, jun., John Neilson.

Letters acknowledging receipt of Proceedings were read from Royal Institution, Liverpool, and the Literary and Philosophical Society of Manchester.

Dr. R. D. Thomson communicated an account of his experiments on the Fluids of Cholera, and on the atmosphere in December, 1848, and January, 1849, which have been published in the Transactions of the *Royal Medical and* Chirurgical Society of London, for 1850.

20th February, 1850.—*The* PRESIDENT *in the Chair.*

THE following were elected members:—Messrs. William Brand, James George Morison, William Rae Arthur, John Burns Bryson, David Cross.

Professor William Thomson gave an experimental demonstration of Mr. James Thomson's theoretical conclusion, that the freezing point of water is lowered by pressure.

The following paper was read:—

XII.—*On the Occurrence of Sugar in the Animal Economy.* By ARTHUR MITCHELL, A.M., M.D.

THE importance of any new fact bearing on the subject of digestion, will, I trust, be received as my apology for reading to the Society the following paper. The subject belongs more peculiarly to the physiological section, but as the aid of chemistry has been constantly called in during the progress of the investigations, and as the works of Liebig have now rendered these subjects more or less commonly understood, I hope it may not be altogether uninteresting to the members generally.

To Liebig, Payen, and to the learned professor of Strasbourg, M. Persoz, as well as to Lassaigne, Bouchardat, Mialhe, and more recently to Bernard and Barreswill, we are indebted for the knowledge of facts with regard to the transformations which the saccharine aliments undergo in the process of digestion, of the highest physiological interest.

Amongst these is one now universally admitted; I refer to the catalytic power, which the salivary and pancreatic fluids possess of converting starch into sugar. Since their researches, however, it has been shown by Magendie that the same property belonged to almost all the fluids of the economy, such as the bile, urine, gastric juice, serum of blood, spermatic fluid, &c. Moreover, on making infusions of portions of brain, heart, lung, liver, kidney, muscle, &c., and adding these to solutions of starch, the transformation was found to be equally complete.

Having observed that the serum of blood acted thus on feculents, after it had been drawn from the body, the same observer was naturally led to inquire if, while circulating in the animal, it could effect the same change. Accordingly, a quantity of starch was injected into the jugular vein of a rabbit, and in less than ten minutes afterwards a portion of blood was withdrawn for examination. Not the slightest trace of starch, however, could be detected; but, apparently in its place, there existed a large amount of sugar. (That the origin of this sugar might not seem to be the food which the animal had last eaten, Magendie had the precaution to take, as the subject of this experiment, a rabbit which had been fasting for three days.)

After the first bleeding, successive quantities were abstracted at intervals of an hour, and subjected to analysis; and it was found that for

the first five hours the quantity of sugar increased rather than otherwise, but after that time it gradually went on diminishing until the quantity became too small for detection. It thus appeared that when starch was injected into the circulation, the blood at once possessed the power of converting it into sugar, and thereafter, by some similar influence, of causing it to disappear.

But as the artificial introduction of starch into the veins is not one of the regular phenomena of life, it became interesting to ascertain if the blood of an animal, nourished on substances containing a large proportion of starch, would indicate the presence of sugar. A dog was therefore fed on a mixture of boiled potatoes and lard for several days, and then, while in full digestion, a vein was opened and sugar readily detected in the blood. The same experiment frequently repeated always gave the same result, and this uniformity naturally led to the following conclusion:—that the existence of sugar in the blood is not, as formerly supposed, a state of disease, but the normal or regular consequence of the digestion of aliments containing starch or sugar itself.

This deduction was the more readily drawn from the notion, which was so generally admitted, that animals have not the power of creating any immediate principle, such as albumen, fibrin, casein, &c.; but only possess the power of appropriating and then destroying such of these as are furnished ready made by the animal or vegetable kingdoms. I say the power of creating sugar, or forming it *de novo*, being thus denied to the animal organism, to maintain consistency it became directly necessary to attribute its presence in blood to the use of feculent or saccharine food.

The matter, therefore, stood thus,—Men had observed that during the digestion of a food containing sugar or starch, the blood of animals contained sugar, and they therefore concluded that it had been furnished by these aliments.

Comparative experiments shall show in how far this was correct.

But before proceeding to their enumeration, I cannot refrain from alluding to the postscript of a paper "on the White or Opaque Serum of the Blood," read to this society by Dr. Andrew Buchanan. He states therein, that his experiments on that subject led him to suspect that the starch might be converted by the organs of digestion into sugar, and be absorbed in that form into the blood. Accordingly, he treated with yeast some serum of blood, which had been withdrawn about three hours after a full meal, and found that fermentation ensued. The same experiment was repeated, and the result again was affirmative of the presence of sugar.

In the second case, however, there was one difference worthy of note,—while the feculent diet had been used more sparingly, the sugar appeared to exist more abundantly. And what seemed still more strange was, that the serum of the blood of the same individual *after fasting* still indicated the existence of sugar, though in small quantity.

This leads us directly to suspect that the occurrence of sugar in the animal economy, is more or less unconnected with the use of the saccharine aliments. And what is here simply hinted at, I shall now adduce experiments to prove.

1*st Experiment.*—A rabbit was fed for several days on a mixture of potatoes, starch, and carrots, it was then killed instantaneously, and blood drawn from the right side of the heart. This was laid aside for coagulation, which was found complete in about one hour. The serum, which was alkaline, was then examined, and I determined in it the presence of sugar in a manner the most positive.

The stomach and small intestine also contained sugar, and traces of starch unaltered.

The urine was turbid, alkaline, and contained no sugar.

2*d Experiment.*—A full grown rabbit was kept without food for two days and then killed. The chest was at once opened, and the blood from the right side of the heart collected in considerable quantity. In less than an hour coagulation was complete, and the serum, clear and alcaline, gave palpable indications of the presence of sugar.

The stomach and small intestine were perfectly empty, and of course contained no sugar.

The urine, which prolonged abstinence as usual had rendered acid, was likewise void of sugar.

3*d Experiment.*—A dog was allowed to fast for a couple of days, and then put for a week on a diet wholly exempt from saccharine or feculent matters. After this, while in full digestion, he was bled from the right side of the heart. On subjecting the serum, which had completely separated from the clot in about three quarters of an hour, to the usual re-agents, I had not the slightest difficulty in detecting the presence of sugar.

I then made infusions of the contents of the stomach and of the chymous mass from the small intestine, but in neither could I find evidence of the existence of sugar.

The same result, negative of the presence of sugar, followed the examination of the urine. The urine gave an acid re-action with litmus paper, as also did the infusions above referred to.

I have repeated these experiments, and varied the manner of performing them, but without affecting the result, and the same has been the case in the hands of other observers.

There can be little hesitation, therefore, in at once drawing the conclusion, to which they so naturally and necessarily lead us, viz. that the occurrence of sugar in the blood of animals is constant and without reference to diet.

One animal was fed on non-azotised food, a second was fed on azotised, a third was subjected to complete abstinence, and in all three sugar was equally detected in the blood. I say we cannot but arrive at one conclusion—*the necessary deduction* from these facts—"That sugar exists

constantly in the blood, whether the animal have used a saccharine diet, an animal diet, or have been subjected to abstinence from food of all sorts."

In the case of the animal forming the subject of the first experiment, the sugar detected in the alimentary canal and stomach might be, and probably was, the source of that found in the blood; but the same cannot be the case with the animal which was fed entirely on nitrogenised food, and whose intestine contained not a trace of sugar; and still less so with the animal which had been subjected to prolonged abstinence, and whose stomach and intestinal canal were perfectly empty.

Whence then, it is naturally asked, came the sugar which existed in the blood of the animals which were nourished on flesh, or denied food altogether? Such is the interesting question which I now proceed, as far as possible, to determine. You have observed that it was invariably found in the blood from the right side of the heart; here, however, it could not have been formed, but must simply have been transported to it from some more or less distant organ. To discover this source, the following experiments were instituted:—

1*st Experiment.*—An adult and healthy dog, having made a copious repast on cooked flesh, was killed seven hours afterwards. On opening the abdomen the phenomena of active digestion were observed. The following were laid aside for examination, viz.:—1st, A portion of the matters contained in the stomach and small intestine. 2d, Some chyle from the thoracic duct. 3d, Blood from the portal vein, by an incision near the point where the splenic vein joins it. And, 4th, Blood from the right side of the heart.

1st, In the contents of the stomach and small intestine no trace of sugar existed. Both were acid.

2d, The serum of the chyle from the thoracic duct was alkaline, but gave no indications of the presence of sugar.

3d, The serum of the blood from the portal vein was slightly lactescent and alkaline, and contained sugar in great abundance.

4th, The blood from the right ventricle of the heart presented a serum also milky and alkaline, and giving indications of the presence of sugar, but in much smaller quantity than in the blood from the portal vein.

2d Experiment.—A healthy and adult dog was killed on the third day of a total abstinence from food of all sorts. On opening the abdomen, such phenomena were observed as always accompany the inactivity of the digestive organs; a paleness and anæmia of all the organs, with vacuity and retraction of the stomach and intestines. The thoracic duct contained a chyle or lymph, which was transparent, or very slightly opalescent.

1. In the first place, blood from the trunk of the portal vein was examined. The serum which separated was limpid and alkaline, and contained evident proofs of the presence of sugar, although certainly in less quantity than in the former experiment.

2. The blood from the right ventricle was then subjected to the usual *re-agents, and gave indubitable* proof of its containing sugar.

3. The lymph from the thoracic duct appeared to contain not a trace of sugar.

These two last experiments were performed by M. Bernard about twelve months ago, and were repeated by him several times with invariably the same results. I have also myself, for further accuracy, performed the experiments under circumstances slightly varied, in order to avoid as far as possible the occurrence of any error. They come, therefore, to be of a nature deserving the greatest confidence. They do not certainly point out definitely the origin of sugar in the blood of animals, fed on azotised food or fasting, which was the question for the solution of which the experiments were instituted, but they draw forcibly our attention to the unaccountable fact, that the vena porta seems to contain in its blood a very large amount of sugar, whilst the contents of the intestine in both cases contained not a trace.

I may here state, for the information of the non-professional gentlemen present, that the portal vein is that which returns the blood from all the chylopoietic viscera, or organs concerned in the formation of the chyle, to be distributed through the liver. It is formed principally by the confluence of the splenic and mesenteric veins, receiving contributions also from the pancreas, duodenum, stomach, and gall bladder. The portal vein is thus made up principally of the veins returning from the intestines, spleen, and pancreas. It seemed, therefore, very singular that the blood of this vein should contain such large quantities of sugar, while the contents of the stomach and intestine were entirely devoid of it. The following experiments were naturally suggested, as likely to throw light on this difficulty.

1*st Experiment.*—Having killed as quickly as possible, that is in some seconds, by division of the spinal bulb, a dog in digestion of matters exempt from sugar or starch, the abdomen was immediately opened, and then, with the greatest possible quickness, ligatures were placed on the following vessels, viz.:—1st, Veinous branches from the small intestine, and not far from the intestine; 2d, On the splenic vein; 3d, On the pancreatic veinous branches; 4th, On the trunk of the vena porta. Then opening these vessels between the ligature and the organ, blood was collected from these different sources: the small intestine, the spleen, the pancreas, and that which flowed backwards from the liver. 1st, In the blood from the intestinal veins, the existence of sugar was rendered evident. 2d, The blood from the spleen gave no indication of its presence, nor did that (3d,) from the pancreatic branches. 4th, In the blood which flowed from the vena porta, very freely, when divided, as in the other cases, between the organ and the ligature, large quantities of sugar were found to exist. On seeing then the blood from the liver containing so much sugar, it was presumable that some would also exist in its tissue. A portion of the liver of this dog was therefore analysed, and sugar detected in great abundance; while the tissues of the spleen and

pancreas, treated in the same way and with equal care, gave no indications of its presence.

2d Experiment.—I bled a rabbit, which had been kept fasting for several days, from the veins which return the blood from the fore and hind legs, and in the serum of neither could I detect the presence of sugar. While in the blood from the right side of the heart, and in the infusion of the liver of the same animal, it existed in abundance.

3d Experiment.—Another rabbit, which had also been subjected to a lengthened abstinence from food, was the subject of the next experiment. A ligature was applied to the portal vein, through as small an aperture in the abdominal cavity as possible. The vessel was then opened on both sides of the ligature, and the blood from each portion laid aside for coagulation, and the same was done with a small portion from the mesenteric artery (the vessel which conveys the arterial blood, or blood *from* the heart *to* the intestines). In the arterial blood, and in that from the vein between the ligature and the gut, no sugar could be detected, or if any, a mere trace. Nor did any proof appear of its presence in the matters contained in the stomach and small intestine. But in the blood from the portal vein, between the ligature and the liver, as well as in infusions of the tissue of the liver, I found sugar in very considerable abundance.

From all this it appears that the liver is in some way the source whence the sugar comes, in such cases at least as those wherein the animal has been confined to a nitrogenised diet, or which amounts to the very same thing, subjected to long fasting.

Sugar is not found in the blood going to the intestine, nor in the blood coming from it and going to the liver, nor in the blood coming from the spleen and going to the liver, nor in that coming from the pancreas and going to the liver, nor in the tissues of the spleen or pancreas, nor in the contents of the stomach and intestine, and yet is found in abundance in the tissue of the liver. I repeat, that from all this it appears that in some way or other the liver is the origin or seat of this sugar, at least in such cases as those wherein the animal had been confined to a nitrogenised diet, or which amounts to the very same thing, subjected to long fasting.

Before proceeding to the examination of this result, I shall cite one other experiment, briefly. *Experiment.*—In rabbits fed on potatoes and beet root, I was able to detect sugar in the blood from any part of the mesenteric veins, as also indeed throughout the whole circulation. But here again it appeared in greater quantity in the liver than elsewhere.

A moment's reflection on what has been written will at once suggest the query,—if the sugar is formed in the liver, how does it find its way back again into the portal vein? We never find in the general circulation that blood, which has already passed a capillary tissue by a progressive movement, ever retrogrades. But this reflux in the portal vein, I conceive more easy of explanation than may be imagined. In a physiological state the portal circulation is mainly dependent on the pressure

exercised on the viscera by the abdominal wall. When the abdomen is opened, this pressure ceases by the escape of the various organs, while at the same time the vessels are dragged out and elongated, and a sort of depletion through the whole length of the vena porta takes place. This vacuum, so to speak, aspires the blood from the liver and other organs, which takes place the more readily that there are no valves to impede the retrogression of the blood. I account, therefore, in this manner for the appearance of the sugar in the portal vein, and I do so the more readily, that Bernard asserts that he has avoided this reflux by the application of a ligature to the vein at its entrance into the liver, before laying open the abdomen.

I consider, therefore, this fact as established, that the sugar in the animal economy is found concentrated in the liver. Whether it exists there from some transformation of the elements of the blood taking place within the organ, I cannot say, but such seems very probable. It may be asserted, however, that it is merely deposited and accumulated in the liver, being *originally* derived from some feculent or saccharine diet. And this opinion is strengthened by the property which the liver is *known* to possess, of retaining in this manner arsenic and other metallic poisons. Indeed, it cannot be denied that the liver does frequently play the part of a condensing or accumulating organ, but in the case in question experiment shows it not to have this property.

Experiment.—A dog was subjected to abstinence both from liquid and solid aliment for eight days, after this time he was supported exclusively and abundantly on cooked flesh, principally boiled sheep's head. On the nineteenth day of his sequestration, the animal was killed while in full digestion. On examination his blood and liver were found to contain sugar as abundantly as in the former experiments.

This was also performed by M. Bernard three times under similar circumstances, and with similar results.

It cannot be imagined that this sugar had been retained during all this time in the liver, for certainly the elimination must have been wholly effected long ere the expiry of the nineteen days. One or two experiments, afterwards to be noted, in reference to the influence of nervous action on these phenomena, will serve to remove any remaining doubt on this subject.

In the discussion of a subject of such importance, it is necessary that every guarantee for the accuracy of the results be given, and I shall therefore now proceed to detail the methods of detecting sugar, which I have employed during these and other investigations. I do not mean to call the attention of the Society to all the tests which have been proposed for sugar, but briefly to enumerate those on which I have placed reliance.

Tests for Sugar.—In searching for sugar in the blood, Trömmer's test is that which is *most* convenient and most sure. There are various ways

of applying it when examining animal fluids, of which I shall enumerate two. 1st, Precipitate the protein compounds by anhydrous alcohol, and add dry carbonate of potash to the filtered spirituous solution. On the addition of a little sulphate of copper and the application of heat, we observe, if sugar be present, a yellow or yellowish brown tint developed, produced by the reduction of the copper to a state of suboxide. This is the method which was employed by Simon in his elaborate researches in animal chemistry, and Trömmer states that its delicacy is sufficient to detect one grain of sugar in 10,000 of blood. I can affirm myself that sugar may be detected in a solution of twice that strength, which is still a state of extreme dilution.

A second method of procedure is the following:—When the blood is extracted from the heart or vessels, it is left to coagulate; then taking a portion of the serum which separates in a test tube, add about a sixth of its volume of the double tartrate of copper and potash; then boiling the mixture, a reduction of the salt of copper will be effected proportional to the quantity of sugar contained in the serum. This mode of operating is very simple, very rapid, and very delicate; and in making comparative experiments it is all that is required. It is that which has been adopted by Bernard, Barreswill, and Mialhe in their various researches on the digestion of feculents.

M. Ferrand has proposed another method, which I find very accurate, but not always readily applied. The blood of the animal is received into boiling water, which separates, by coagulation, the albumen and fibrin, and retains the substances soluble. The liquid is filtered, rendered neutral by some drops of acid, and evaporated gently; the residue treated with alcohol, &c. as in the former cases.

Although this test, employed in either of these three ways, is most valuable in comparative experiments, yet for additional security occasional recourse must be had to others. Among these, the fermentation test decidedly stands first. A small quantity of barm is added and the gas collected in a suitable apparatus. If the quantity of sugar be too small to give the products of fermentation sufficiently distinct, various plans are employed, and one of the best is that proposed by Dr. M'Gregor of Glasgow. The serum is coagulated by heat, and carefully dried on a steam bath. The solid clot is divided as minutely as possible and boiled in water; this is then filtered and evaporated to a certain extent. To the concentrated fluid the yeast is then added. When the fermentation test is applied in its widest bearings, I conceive it to be absolutely conclusive of the presence of sugar. If the gas given off be proved, by suitable tests, to be carbonic acid, and if the liquid left be shown, by distillation, to contain alcohol, I think all will assert that sugar must have existed in the fluid. And such proof have I of the occurrence of sugar in the liver. The specimen of spirit which I have in my hand, sufficiently concentrated to be inflammable, is the result of the distillation of a calf's liver. It was *purchased in the* market immediately after the animal was killed, and

fermentation, as soon as possible, established in an infusion. When the process was completed, it was distilled and redistilled till I obtained what I now present to you.

There is another application of the fermentation test—I refer to the production of the Torula Cerevisi, which can be readily and positively recognised by the microscope, when the examining eye is one accustomed to the use of the instrument.

In quantitative analyses, I have estimated the sugar by the amount of carbonic acid given off, reckoning one cubic inch of gas as equivalent to one grain of grape sugar, or by more accurate calculation, 100 C. I. of CO_2 correspond to 106.4 of sugar. Or if the CO_2 be estimated by weight, one grain of the gas will be found equal to $2\frac{1}{4}$ grains of sugar.

One thing of importance has to be attended to in searching for sugar in the blood, viz. that sugar is destroyed in, and disappears from, the blood, after being drawn with great rapidity, so that it becomes necessary to act on the serum as soon as ever the coagulation is sufficiently complete. In order to prevent this destruction it is only requisite to coagulate the blood as it escapes from the vessel by alcohol or acetate of lead, after which the sugar will remain unchanged for a considerable period.

As regards the variety of sugar which exists in the animal economy, we may conclude that it is neither sugar of milk nor cane sugar. It cannot be the sugar of cane, for it is rendered brown by the action of potash, and reduces the salts of copper; nor, since it ferments readily, can it be the sugar of milk. There remains, therefore, the grape sugar, and of this the sugar of the liver presents the chemical characters. The optical experiments of M. Biot show the sugar of diabetes to be identical with the sugar produced from starch. It is possible, therefore, that this animal sugar may possess certain differences in its properties, although it agrees in all essentials with the grape sugar. Indeed, there is some reason for believing that this will by and by be established.

I shall now, gentlemen, recapitulate the conclusions, which, to my mind, seem the necessary deductions from the foregoing considerations:—It would appear, in the first place—1st. That sugar exists uniformly and normally in the blood of the heart; I say the blood of the heart, because it will be shown afterwards that it may have all but disappeared before arriving at the superficial veins of the body, where bleeding is usually practised.

2dly. That its presence there is independent of diet.

3dly. That the sugar is found specially concentrated in the liver of animals.

4thly. That there is reason to believe that it is *formed* in the liver, which thus becomes at once the seat and origin of the sugar.

5thly. That in the use of a saccharine diet, sugar enters the circulation directly as sugar.

And lastly, that these considerations oblige us to reject the doctrine that animals *do not create* any immediate principle, but simply destroy

those supplied by the vegetable kingdom, for we have found them both forming and destroying sugar.

It does not follow, that because the animal organism seems thus to possess transforming powers, which we cannot command in the laboratory, that chemistry is to be dismissed from the study of the phenomena of life. On the contrary, I believe that it alone can, in many cases, remove the difficulties which arrest the progress of physiology; but I am also of opinion that, in order to the successful prosecution of such investigations, they must enter the field and work conjointly. The saliva, &c. possess the power of converting starch into sugar without any reference to whether chemists know or are ignorant of their having such properties; and although we have not yet discovered them, many other catalytic influences may be effecting their transformations in the organism, and amongst them may be one capable of converting the oleaginous into saccharine and and albuminous matters.

The simplest conception of the saccharine principle is an association of water and carbon, and in this light it may be regarded as the intermediate link between inorganized and organized matter. This union of water and carbon, and the consequent formation of the saccharine principle is effected by the lowest vital agency with which we are acquainted —that of plants. It may occur, as Prout believes, from a direct union between these substances; but it seems to take place most usually by the aid of a collateral extrication of oxygen, during which the unfettered carbon is appropriated. When this association is simple, starch or sugar is the result; but when more complicated, and nitrogen, phosphorus, and sulphur are involved, various compounds are produced, which differ in their properties between sugar and albumen.

It has not yet been shown, whether the vital energies of plants can convert oleaginous into saccharine and albuminous matters; but the vital organs both of plants and animals appear capable of performing the reverse act, that of changing saccharine into oleaginous matters, and this is probably the usual mode in which oils are formed in plants and animals.

The union of water with carbon, and afterwards with nitrogen, has been maintained to be the peculiar function of plants. There seems reason for believing, however, that it is not limited to them. Prout, who holds this opinion, gives this singular paragraph, written more from an apprehension of what he felt would eventually be discovered, than from what he himself knew at the time:—

"In all animals there is a vegetative organ, (if we may be allowed the expression,) capable in a greater or less degree of performing the same functions as vegetables, i.e. of combining water with carbon; *or*, if not beginning at this low point of the scale, at least of combining the organized saccharine principle with azote, &c. so as to form albuminous products. This vegetative organ is the *liver;* and though the vegetative faculty alluded to appears to exist in the livers of different animals in very different *degrees, yet in no instance* is it entirely wanting. In all the more perfect,

and particularly in carnivorous animals, when appropriately fed, this function is little called into action, and its existence therefore is probably intended merely as a resource to fall back upon in case of necessity. Without it, however, animal life would be most precarious, or, in many instances, even impossible."

With these remarks I shall conclude the first part of my subject, and proceed at once to the examination of the great and important question which at once presents itself as now requiring an answer:—How, or by what agency, and in what part of the system, does this sugar disappear from the blood?

I have hitherto dealt with my subject in a pure chemico-physiological light, nor do I purpose doing differently in that part of the paper which is to follow. I may state, however, that these researches have been undertaken by me, and possess an interest to my mind, in as far as they may possibly lead to some rational, and I hope successful, method of treating that most distressing disease, wherein sugar appears in the urine, and which has hitherto been regarded as beyond the reach of the vis medicatrix.

SECOND PART.

How is this sugar which has been shown to exist in the blood and liver caused to disappear? How is it destroyed? By what agency, and under what influences? What are the products of its transformations, and in what part of the system do they take place?

Such is the problem, in the attempt to solve which I have been for some time occupied. I proceed at present to lay before you a few facts bearing on this interesting subject.

The changes which sugar undergoes when brought into contact with other bodies, having a marked influence on it, are not confined to any narrow limits, like those of inorganic bodies, but are, in fact, unlimited.

In inorganic compounds, we find that acid acts upon a particular constituent of the body, which it decomposes by virtue of its affinity for that constituent, and its proper chemical character is maintained in whatever form it be applied. "But when the same body acts upon sugar, producing great changes in that compound, it does this, not by any superior affinity for a base existing in the sugar, but by disturbing the equilibrium in the mutual attraction of the elements of the sugar amongst themselves. Muriatic acid and sulphuric acid, which differ so much from one another both in properties and composition, act in the same manner upon sugar. but the action of both varies according to the state in which they are; thus, they act in one way when dilute, in another when concentrated, and even difference of temperature causes a change in their action. Thus, sulphuric acid of a moderate degree of concentration converts sugar into a black carbonaceous matter, forming, at the same time, acetic and formic acids. But when the acid is more diluted, the sugar is converted into two brown substances, both of them containing carbon, and the elements

of water. Again, when sugar is subjected to the action of alkalis, a whole series of different new products is obtained; while oxidising agents, such as nitric acid, produce from sugar carbonic acid, acetic acid, oxalic acid, formic acid, and many other products not yet examined. If, from the facts here stated, we estimate the power with which the elements of sugar are united together, and judge of the force of their attraction by the resistance which they offer to the action of bodies brought into contact with them, we must regard the atom of sugar as belonging to that class of compound atoms which exist only by the vis inertiæ of their elements. Its elements seem merely to retain passively the condition in which they have been placed."

It has been shown that the blood of animals contains sugar; and the same substance, as is well known, exists also in the sap of plants. Instead, however, of being destroyed and disappearing from the sap of plants, we find it deposited in some particular part, or aggregated in the general tissue. Now, in what respects does the sap of vegetables most ostensibly and uniformly differ from the blood of animals? When in a healthy condition the blood is an alkaline fluid, while the sap of such plants is either neutral or acid, and never alkaline. But will the difference in this property account for the destruction of the sugar in the one case, and its being hoarded up in the other? Let us change the respective conditions and mark the effect.

If the acidity of the sap of the vegetable be modified by watering it with a slightly alkaline solution, it acquires chemical properties analogous to those of the blood, and we find, as an apparent result, that the sugar is destroyed as rapidly as formed, that the secretions are no longer saccharine, and that it no longer bears sweet fruits. This fact has been established by M. Frémy.

And now, in cases of diabetes, where sugar, ceasing to be destroyed, passes off in the urine, let us inquire, if the change this supposition would predict has really taken place—if, instead of being alkaline, the blood is neutral or acid.

We find certainly the saliva of diabetes acid, and the humours generally more acid than normal; but with regard to the blood, I believe that in this disease it is often found neutral, very rarely acid, and generally alkaline. It may still, however, be that the alkalinity is diminished in degree, and in this manner its healthy functions may be impeded, sufficiently to give serious results.

These facts, standing alone, incline us to suspect that the alkaline condition of the blood is active in effecting the destruction of the sugar, either in part or in whole; and here, in the meantime, I shall quit the consideration.

Do we derive any information on this subject from comparative analyses of blood from different sources? I fear not, from any hitherto performed, which are sufficiently extensive and accurate to yield a fair average and *authorize a deduction.* I shall briefly state, however, the averages of

some such analyses, more as showing the necessity, in all analyses of blood, for stating the point of the circulation from which the blood was drawn, and the circumstances, as regards diet, under which the animal existed at the time of the experiment; I say more for these reasons, than that I conceive them to have any practical bearing on the solution of the question in hand.

Three dogs were bled, each from the jugular vein and vena porta, and the six portions of blood were analysed. No two of the analyses were identical, but the differences all went in one direction; and I give the following as an average of the three: That from the jugular vein contained in 1000 parts 769.21 of water and 230.78 of solids, while that from the portal vein gave 726.54 of water and 273.46 of solids, *showing an excess of nearly 4½ per cent. in the solids of the portal blood.* (This excess consisted mainly of the globules and fibrine.) These three animals had been fasting some time before losing these specimens of blood; and I now give you the average of five other experiments, in which the dogs, at the time of death, were in active digestion of fluids and solids. In these, the blood from the jugular vein gave, in 1000 parts 780.92 of water and 219.08 of solids, and the portal blood 790.11 of water and 209.89 of solids, *showing, under these altered circumstances, a loss of solids, where an excess existed when the animals were fasting.* The precaution, to which I alluded, is surely inculcated in these results.

I now would inquire if sugar exists in the same proportion in all classes of animals, under conditions as similar as possible, and I find that in birds and mammiferous animals the amount is alike very considerable. In reptiles, such as the frog and lizard, the sugar existed merely as a trace, while in fishes, as in the skate and eel, not a trace could be found. Whence comes this disappearance of sugar in cold blooded animals? Does it arise from the diminished energy of the respiratory functions?

This query leads us to the path, by diligently following which, I believe, we shall arrive at the explanation of this important phenomenon.

"Several circumstances have induced recent writers to conclude that nitrogenised foods are alone capable of conversion into blood and of forming organized tissues; that, in fact, they only are the foods properly so called, and hence have been denominated by Liebig *the plastic elements of nutrition.* The non-nitrogenised foods, it is said, are incapable of transformation into blood, and are therefore unfitted for forming living tissues. They are, nevertheless, essential to health; and Liebig asserts that their function is to support the process of respiration, (by yielding carbon and hydrogen, the oxydation of which is attended with the development of heat,) and some of them, he states, contribute to the formation of fat. These non-nitrogenised foods he calls *the elements of respiration.*"

It would appear, then, at all events possible *that respiration is actively concerned in bringing about this destruction of the saccharine principle.* If so, we shall probably find some change in the expired air, a diminution

or excess in some of its ingredients. Accordingly, I have carefully estimated the amount of CO_2 in the expired air of several diabetes; I have done the same with the expired air of healthy individuals, after a similar diet, and otherwise under conditions as nearly as possible the same. I have searched in both for the presence of other ingredients. I have endeavoured to examine and state comparatively the condition of the cutaneous respiration in diabetes and in healthy persons. I have inquired if the temperature of diabetics falls below the normal standard. I have examined the blood and urine of persons in whom the respiratory act was incomplete, from a morbid condition of the lungs. I have done the same with individuals who had been long in a state of anæsthesia, from the inhalation of chloroform, and in whom the oxygenation of the blood must necessarily have been incomplete. I have compared the blood before entering the lungs with that which had passed through them. I have impeded respiration by division of the pneumogastric nerves, singly, doubly, and in different localities, and have then searched for the result in changes of the blood, urine, &c. I have irritated various portions of the brain, which I thought might affect the same. In short, I have cross examined nature in every way which I thought might extort the truth. The answers I have hitherto received lead me towards certain inferences; but I do not yet consider the experiments sufficiently multiplied to warrant the announcement of deductions, especially as the subject is one of such high importance. I shall continue to prosecute them; and I hope on some future occasion I may have the honour of communicating the results to this Society.

P.S.—Before beginning a series of researches, (having for their end the discovery of the manner in which the sugar, constantly entering the circulation, is removed therefrom,) I deemed it right to establish the accuracy of the conclusions arrived at by other experimenters on allied subjects, and which required to be received as true at the outset of my investigations.

The first part of the foregoing paper contains the results of a train of experiments instituted with this object.

Free reference has been made to the works of the following observers: Bernard, Barreswill, Mialhe, Magendie, Liebig, Persoz, Müller, &c. &c.

March 6th, 1850.—*The* President *in the Chair.*

It was agreed, on the motion of Mr. Liddell, that a deputation should be sent from the Society to the meeting of the British Association to be held in Edinburgh in August.

Dr. Allen Thomson gave an account of recent observation respecting the germination of the Ferns.

The following paper was read on the parallel-roads of Glen Roy.

XIII.—*On the Parallel Roads of Lochaber.* By JAMES BRYCE, Jun., *M.A., F.G.S.*

I.—INTRODUCTION.

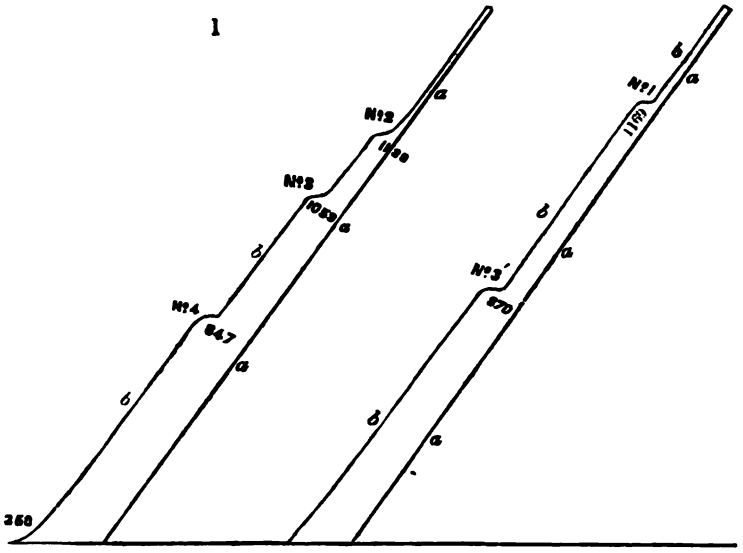
THE Lochaber glens have been subjected to so keen a scrutiny by the advocates for the various theories of the Parallel Roads, that it cannot be expected there should remain many facts of importance to be yet ascertained. By this circumstance, however, the obligation upon an observer at once to make known such facts as may have come under his notice is rendered more imperative, while the value of new facts is enhanced. Observations, which in other circumstances would be scarcely deemed worthy of record, become of importance when viewed in connexion with an inquiry such as this, which, after all the discussion elicited by it, still remains the great unsolved problem of Scottish geology. In submitting the following communication, it is not my purpose to advance a new theory. I have merely in view the much more humble object of putting on record a few facts, which seem to have escaped the notice of previous observers; and of offering, in connexion with these, some remarks on the two theories last proposed. I refer to those of Mr. Chambers of Edinburgh, and Mr. James Thomson of Glasgow, both published early in 1848; the latter immediately before my visit, which took place in July of that year. My examination of the district had thus additional interest given to it, as the facts were to be viewed under a somewhat novel aspect, and had not yet been commented on by any geologist, with reference to their bearing upon the two theories in question.

Before proceeding, however, to remark on these theories, it will be necessary to state the principal facts which have been ascertained respecting the Parallel Roads.

II.—ABSTRACT OF FACTS.

The Parallel Roads are shelves or terraces on the sides of certain glens in Lochaber, perfectly parallel to one another and to the horizon, throughout their entire course, and at exactly the same height on opposite sides of each glen. They conform to all the windings of the hill slopes, their continuity being broken only by rocky projections, and by the lateral streams. The breadth is various, generally from 8 to 10 yards, in a few rare cases reaching to 18 or 20, owing to the peculiar form of the ground; but the precise width is difficult to ascertain, in consequence of the outer edge of the shelf being rounded off towards the valley. There are five principal shelves in the district, besides some minor ones. They are most distinctly marked in Glen Roy and Glen Gluoy. There are three in the former glen and two in the latter. The upper shelf in Glen Gluoy is called No. 1, and the highest, middle, and lowest in Glen Roy, No. 2, No. 3, and *No. 4, respectively.* The second Glen Gluoy shelf having been

very recently discovered, is not yet designated by any number. There is another well-marked shelf, also recently discovered, near Kilfinnan, at the northern end of Loch Lochy; and in various parts of the district there are traces of higher, and also of intermediate shelves. All these are laid down upon the map referred to in the next section. The principal shelves are also marked on Johnston's Map of Scotland. Their situation is shown in the annexed diagram, No. 1.

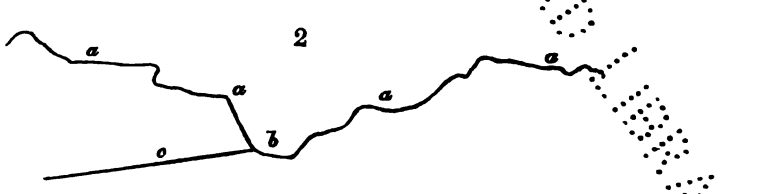


Shelf No. 4 is 847 feet above the sea, or about 500 feet higher than the opening of Glen Roy, which is about 347 feet above the sea. Shelf No. 3 is 212 feet higher, or 1059 feet above the sea. No. 2 is 80 feet above No. 3, or 1139 feet above the sea. Shelf No. 1 in Glen Glouy is 30 feet higher than No. 2, or 1169 feet above the sea. The lower Glen Gluoy shelf is about 210 feet below the upper, or 870 feet above the sea, and therefore 23 feet higher than No. 4 in Glen Roy. It might hence be designated by the number 3′ or 3½, being intermediate between No. 3 and No. 4. * The shelf at Kilfinnan is 40 feet higher than No. 1, or 1209 feet above the sea.

In Glen Roy each shelf runs farther towards the mouth of the glen than the one above; thus, No. 3 terminates farther down the glen than No. 2, while No. 4 not only runs farther down the glen than No. 3, but passes outside the glen, and can be traced on both sides of Glen Spean

* These heights are given on the authority of Robert Chambers, Esq., to whom geologists are much indebted for the careful measurements obtained by him of a *great many points* in the Lochaber district.

to within 6 or 7 miles of Fort-William. In Glen Gluoy, on the contrary, the upper shelf extends farther down the glen than the lower. Each shelf is on a level with some watershed, that is, with some *col*, or landstrait, or lowest part of the ridge dividing two glens, as in the annexed sketch, No. 2.



a a Ridge dividing two glens. b Lowest part of ridge, or col. c Shelf.

Thus, shelf No. 1 is on the level of the passage leading from Glen Gluoy into Glen Toorat, which branches off Glen Roy near its upper end. No. 2 stops at the extreme north-eastern angle of Glen Roy, near the level of the opening into Strathspey; and so of the other shelves, as expressed by the arrows on the map referred to in the next section. The only exception is the second Glen Gluoy shelf, (No. 3′,) which is not on a level with any watershed.

Up to so recent a date as 1817, the Parallel Roads were regarded as works of art; but it is now agreed on all hands that they are due to natural causes. If we suppose that, in a former condition of things, the sea penetrated to these glens, or that, the mouths of the glens being blocked up by earthy materials, or by ice, the waters of the rivers accumulated behind so as to form lakes, then, the action of the water on the alluvial coating of the hills, and on the earth and stones which descended from the heights and were arrested and re-arranged at the margin, would form a beach line such as we now see upon most shores when the water stands a little lower than usual. The shelves thus mark the successive levels of the water as the sea retired on each upheaval of the land, or as the lakes sank to successively lower levels, by the partial disruption of the barriers. The *cols*, or passages between the glens, coincident with the several shelves, are, according to one theory, the channels or straits between islands; in the other they mark the levels where the redundant waters flowed out from glen to glen, during the time that the lakes were forming the several terraces. In confirmation of the latter view, it has been shown that there are at the *cols* several deserted river channels, having no reference to the present drainage.

III.—Error of the Maps.

Mr. Chambers' account of the Parallel Roads, with his theory of their origin, forms a portion (pp. 95–130) of his valuable and beautifully illustrated work on Ancient Sea Margins. A map of part of Lochaber, showing *the shelves in the glens*, is given at the end. It has been "con-

structed by Messrs. W. and A. K. Johnston, under the direction of Sir George M'Kenzie, Bart., David Milne, Esq., and Robert Chambers, Esq." The same map accompanies a late paper on the Parallel Roads, by Sir George M'Kenzie; (Ed. N. Phil. Journ., vol. xliv.;) it is that to which Mr. Milne refers in his late important paper, (Ed. N. Phil. Journ., vol. xliii. p. 339,) and on which the reasonings of Mr. James Thomson are founded, an enlarged copy of it having been laid before the Royal Society of Edinburgh along with his paper.

Now, this map contains an important topographical error, calculated to mislead those who may frame theories of the Roads without having made a personal inspection of the ground. The error consists in this—that at its junction with Glen Fintec, Glen Gluoy is laid down as opening towards Loch Lochy; whereas, in point of fact, the high ridge descending from the table-land at the top of Glen Toorat, and shutting in Glen Gluoy on the west, continues its course southwards fully a mile below the point where Glen Fintec opens into Glen Gluoy. Glen Fintec is *thus completely cut off from direct connection with* Loch Lochy, the ridge in question being continuous throughout, and rising to the height of from 1200 to 1800 feet above the sea, or from 300 to 700 feet above the upper shelf. The rocks of which the ridge consists are chiefly micaceous slate and quartzite, the strata being nearly on end, and ranging in the direction of the ridge, or about S.W. I could detect no traces of scratching or grooving, though the rocks are laid bare in many places, and strew the surface in huge flat masses.

The error now pointed out involves another in the representation of a portion of the upper shelf. The eastern portion is correctly represented as terminating at the south-west corner of Glen Fintec; but on the west side, the shelf, instead of terminating as expressed on the map, is continued a considerable distance southwards of the opening of Glen Fintec; from half a mile to a mile, or perhaps more; at first less distinct than usual, then more plainly marked, till coming against a rocky projecting ledge on the hill side, it fails as usual to impress it, and is seen no more.

On referring lately to Sir T. D. Lauder's map accompanying his paper, (Trans. Roy. Soc. Edinb. Vol. IX.,) which I had not looked into before visiting the Parallel Roads, I found that his representation of this portion of the district is much more correct. Glen Glouy is given in its true dimensions; and the stream formed by the union of the Gluoy and Fintec waters is laid down as turning, at a place called Lowbridge, round the southern termination of the mountainous ridge just described, and discharging into Loch Lochy, nearly opposite to a village named Kyle-Rose in Mr. Chambers' map. This representation is very near the truth; but perhaps too great extension is given to the southern part of Loch Lochy.

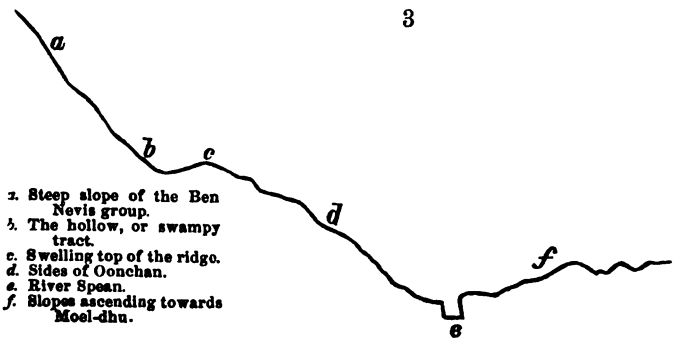
IV.—MR. ROBERT CHAMBERS' THEORY.

One of the principal objections which has been urged against Mr. *Milne's theory, is* the absence from the district of a sufficient quantity of

detrital matter to account for the barriers at the mouths of the glens, required by the theory. The force of this objection would be very much diminished, if we could receive Mr. Chambers' account of the hill of Oonchan, as correct. It appears to me, however, that he quite over-estimates the amount of detritus in this hill.

After giving a full and accurate description of the other principal detrital accumulations of the district, Mr. Chambers thus notices the hill of Oonchan:—"By far the grandest delta of the district is that hill which has been referred to under the name of Unichan as occupying so much of the lower part of Glen Spean. This is a mass of gravel 11 miles long by perhaps 2 broad, reaching an elevation of 612 feet. I observed rock rising through it at one place; but it is mainly, as has been said, a hill of gravel." He considers that, "when the sea stood somewhat above 622 feet (and there is evidence of its having paused long at 628 or 630) the rivers descending from the Ben Nevis group of mountains delivered their spoils into the estuary filling Glen Spean: on the withdrawal of the sea this mass was left."

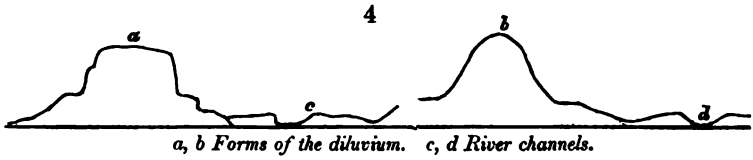
The high ground in question, part only of which is called Oonchan, is an undulating ridge parallel to the main chain, and stretching from near Fort-William to within 1½ miles of the bridge of Roy, a distance of about 12 miles. Such subordinate elevations are seen at the base of almost every high chain, and mark the axes along which the upheaving forces acted with decreasing intensity. This ridge is separated from the main chain by a slightly depressed tract, having a very smooth outline, into which five glens, descending from the Ben Nevis group, open at right angles, the surface presenting no marked change of character at the junction. The streams from these glens, as well as those which drain the tract itself, being prevented by the high ground in front from following direct courses to the valley of the Spean, are deflected to the east and west, parallel to the high ground on either side. The watershed of the tract being nearer the western than the eastern end, and the inclination eastwards slight, there is an imperfect discharge of the waters, and consequently extensive swamps have been formed, which sometimes become



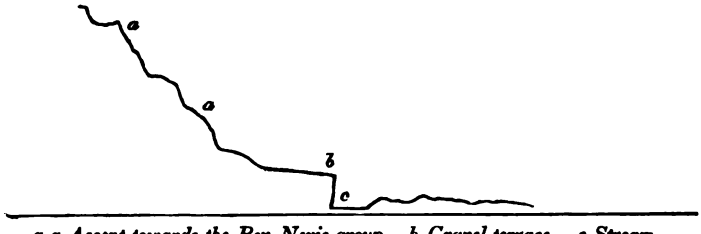
lakes. The annexed sketch, No. 3, will give an idea of the outline of the surface.

On the western part of the ridge the rock is seen in many places; and about the middle I found it a little lower than the highest point, c, of the ridge at that part; and I think there can be little doubt that the thickness of the detrital covering is in most places inconsiderable. At its eastern termination detritus appears in more imposing quantity. Near the bridge of Roy the end of the ridge is cut through by numerous streams, or rather the channels of streams, for there is often no water; and the detritus stands out in numerous round or elliptic flat-topped mounds with steep sides. Towards the base of Cruachaninish and Benchilinaig these are smaller and rounder, resembling Danish raths; while further back the detritus only shows in terraces, formed by the streams cutting into the talus at the base of the high mountains; as in Nos. 4 and 5.

4



a, b Forms of the diluvium. c, d River channels.



a a Ascent towards the Ben Nevis group. b Gravel terrace. c Stream. d Abraded surface.

Mr. Chambers regards the question of the origin of the Parallel Roads as "involved in that of the superficial formations generally, which bear the marks of former levels of the sea at various intervals up to 1200 feet;" the various markings in the three kingdoms, in France, &c., "all falling into such conformity as to prove that the shift of level has been effected from at least that height, with perfect equability throughout." He considers this widely extended and strongly marked conformity "as more favourable to the idea of a recession of the sea, as opposed to that of an elevation of the land, since it is precisely what would result from the former operation, while there is an obvious difficulty in supposing" that so large a portion of the earth's crust could be repeatedly upheaved, and yet the relative levels so preserved that "between Paris and Inverness not a vertical foot of derangement could be detected."

The explanation of the origin of the Parallel Roads is thus mixed up with, indeed forms an essential part of, his general theory. And whatever difficulty geologists may feel in giving their assent to such generalizations as those just quoted, or however unwilling they may be, in the

present state of inquiry, to admit many successive equable sinkings of the waters of the ocean all over the globe, the same difficulties and hesitation must be experienced in receiving Mr. Chambers' explanation as the true theory of the Parallel Roads. Besides, the *speciality* of the phenomena is by no means accounted for on this hypothesis. It appears to me to require a special local cause. On the hypothesis of the shelves being formed by the sea, it cannot, I think, be shown why other Highland glens were not equally impressed; or that any conservative influences have operated in Lochaber, which were not just as likely to prevail in other places. This argument cannot be properly estimated by one who has not seen the shelves in Glen Roy and Glen Gluoy; from examining sea and lake-terraces, from descriptions and drawings, the faintest conceptions only can be formed of the wonderful reality. Any one on whose view the scene which is presented on turning the flank of Bohuntine hill, bursts for the first time, must look with the deepest astonishment at the distinctness, continuity, and extent of the shelves; he will feel how inadequate were all his conceptions, and how little the Parallel Roads have in common with any appearances which have come under his notice before. Mr. Chambers eloquently describes the first impressions, and acknowledges the "singular distinctness" of the shelves in this locality; yet his theory affords no explanation of a phenomenon so remarkable. But this argument has been so ably handled by Mr. Milne in his reply to Mr. Darwin, (Ed. N. Phil. Journ., vol. xliii. p. 437,) that it is unnecessary to insist further upon it.

The faint and higher markings on the south side of Glen Spean, which Mr. Chambers lays so much stress upon as supporting his view, I did not notice. "The whole," he says, "might appear doubtful to many persons; in an unfavourable light, a hasty observer might pass them by altogether unnoticed." These may have been my circumstances, and I do not therefore question the existence of such markings; but I cannot regard the conclusion as warranted by the facts—the existence, namely, "in Glen Spean of a body of water at levels above the barriers assigned to it by M'Culloch, Lauder, and Milne." Are not these and similar slight and local markings best explained on the received theory—original inequalities, the action of currents upon the submerged land, or occasional pauses in the process of elevation?

While thus dissenting from the theoretical conclusions at which Mr. Chambers has arrived, I cannot forbear to express my high admiration of his patient and active research,—his clear, truthful, and eloquent descriptions,—and of the service he has rendered to geology by his many exact measurements, and by proposing a theory which will lead to a more careful study of phenomena of this class.

V.—Mr. James Thomson's Theory.

The lake theory has gained immensely of late by the advocacy of Mr. David *Milne. His paper,* already referred to, is perhaps the most able

which has been written upon the Parellel Roads. The evidence in support of his own views has been collected with the greatest sagacity, and the arguments founded upon it conducted with consummate skill; while he appears to me to have completely demolished both the theory of Mr. Darwin, and the glacial theory, *in the form proposed by M. Agassiz.* The agency assigned by Agassiz will not explain all the phenomena, and is positively inconsistent with many facts. But it does not hence follow that glacial action is to be rejected, as explaining the blocking up of the mouths of the glens,—for it is required for this purpose alone. May not a form be given to the theory which will adapt it to all the exigencies of the case, and thus remove from the lake theory the one great remaining objection—the origin and the disappearance of the enormous earthy barriers at the mouths of the glens? Since Agassiz wrote, the question has been placed on a very different footing. The first glacialist in Europe, Prof. J. D. Forbes, has given it as his decided opinion that glaciers formerly existed on the Cuchullin hills in Skye (Ed. N. Phil. Journ., vol. xl. p. 79). Why, then, may not masses of ice have filled the still higher valleys of the Ben Nevis group of mountains? Professor Forbes' late discoveries in Switzerland respecting the viscidity of glacier ice, and the nature of glacier motion, appear to have suggested to Mr. James Thomson the highly ingenious modification of the glacial theory lately proposed by him (Ed. N. Phil. Journ., vol. xlv. p. 49). The gist of this theory is contained in the following passage:—

"In Switzerland the mean temperature of the comparatively low and flat land is so much above the freezing-point, that the ice no sooner descends from the mountains than it melts away; and it is thus usually prevented from spreading to any considerable extent over the plains. In the Antarctic continent, on the contrary, the mean temperature is nowhere so high as the freezing-point. The ice, therefore, which descends from the hills unites itself with that which is deposited from the atmosphere on the plains; and the whole becomes consolidated into one continuous mass, of immense depth, which glides gradually onwards towards the ocean. . . . . Now a climate somewhere intermediate between these extremes appears to be that which would be requisite to form the shelves in the glens of Lochaber. The climate of Switzerland would be too warm to admit of a sufficient horizontal extension of the glaciers; that of the Antarctic continent too cold to allow the lakes to remain unfrozen. If the climate of Scotland were again to become such that the mean temperature of Glen Spean would be not much above the freezing-point, there seems to be every reason to believe that that glen would again be nearly filled with an enormous mass of ice; while its upper parts, and also Glen Roy, would be occupied by lakes. . . . ."

The state of things here supposed is extremely critical; not likely long to maintain itself under the same geographical distribution of the surface as now prevails, and liable to be changed by many slight causes. If the *mean temperature* of Glen Spean was little above freezing, and wide fields

of ice covered its surface, it is not probable that the lakes in the glens, at considerably higher levels, would long remain unfrozen; and if the Ben Nevis group of mountains, whose mean height we may take at somewhat less than 4000 feet, not only nourished glaciers in their higher recesses, but were wholly enveloped in sheets of ice, can we suppose that the mountains surrounding Glen Roy and Glen Gluoy, many of which attain the altitude of from 2000 to 2500 feet, would not likewise give origin to masses of ice, descending into the glens, and occupying the very sites of our supposed lakes? On the other hand, it may be stated in favour of Mr. Thomson's views, that the hypothesis of Glen Spean being "filled with an enormous mass of ice" which would block up Glen Roy, is more consistent with the geography of the district, than the supposition that a glacier descended from one of the high valleys of the Ben Nevis group, and forced its way into the opening of Glen Roy. There is nothing in the nature of the country to determine a glacier to follow such a course. The form of the surface between the Lochaber glens and the Ben Nevis group is such, that if a glacier descended from any one of the five great glens, whose directions are inclined to that of Glen Roy at an angle of 60 or 70 degrees, and reached the open country at the base of the mountains, there would be nothing to determine its course up Glen Roy, or indeed in any one direction more than another, except the slight eastward and northward slope already described. Glaciers descending from these glens would thus coalesce into one huge sheet, coextensive with the valley of the Spean. The hypothesis of sheets of ice covering the whole surface —"des grandes nappes de glace"—seems also more consistent with the absence of "perched blocks" and moraines, than the idea of separate glaciers. These are not seen anywhere over the surface of the open tract between the mountains and the river; and the peculiar detrital covering is very like that which would be formed under such advancing sheets, most of it being stratified sand and small gravel, the result of wearing, or decomposition *in situ*.

Mr. Thomson's explanation of the phenomena of Glen Gluoy is very ingenious. It will be remembered that these are peculiar. The shelves do not correspond with those in the other glens; and while in the latter each successive shelf, as we descend, extends further down the glens than those that are higher, in Glen Gluoy the upper shelf extends further towards the mouth of the glen than the lower; and this lower shelf, unlike all the others, is not in connexion with any summit level. If the lake theory be true, it will follow from these facts that the barrier which retained the water at the lower level was further up the glen than that which retained it at the higher; and that when the lower shelf was forming, the overflow must have taken place at the mouth of the glen. Mr. Thomson supposes "that the glacier which occasioned the formation of the higher of the Glen Gluoy shelves, had at some former period protruded a *terminal moraine as far* up the glen as the termination of the lower

shelf; that, on the final retiring of the glacier, this old moraine served as a barrier to dam up the water to the level of the lower shelf, and that it has been subsequently washed away by the river flowing over it." He then suggests that the space between the terminations of the upper and lower shelves should be examined, to ascertain if the remains of such a moraine exist. I made this examination with considerable care, but could find no such remnants. There is some detritus in the main glen opposite the mouth of Glen Fintec; but it has obvious reference to the present drainage, and is in no way remarkable. The whole of Glen Gluoy is indeed singularly free from detritus;—a peculiarity which I consider due to its form. It is narrow, and the hills rise steep and high from the very margin of the river, so that there is no space where detritus could rest; and it is thus swept away as soon as it is brought down. This circumstance is also favourable to the rapid and complete removal of such a moraine, or barrier, as Mr. Thomson supposes may have once existed. The mouth of the glen is equally free from detritus, or other indications of the existence of earthy barriers in a former condition of things.

"A glacier occupying the present site of Loch Lochy, and receiving supplies from the neighbouring mountains, would appear," Mr. Thomson says, "to afford a sufficient explanation of the phenomena observed in this glen." This was no doubt written under the impression that Glen Fintec communicated with Loch Lochy, and that the mouth of Glen Gluoy was in the way of a glacier advancing from that lake. But this is not the case. A glacier having its origin among the high mountains to the N.W. of Loch Lochy—the only hills high enough to produce one—and *advancing from Loch Lochy*, must make its way past Maucomer and Brecklech up the valley of the Spean, for so only will the levels permit. This direction is about perpendicular to that of Glen Gluoy; and it would be only a lateral branch or arm, parting from the main body, that could penetrate that glen. The mouth of the glen is narrow, and the hill sides rise steep and high; a little way up there is a considerable bend before we reach, at a mile's distance, the bosom or *sinus* in the hill side, where the moraine is conceived to have existed in connexion with the lower shelf. All this shows the improbability of a moraine being deposited at this place by such a glacier; and that recourse may as well be had to the masses of ice with which Glen Spean has been supposed to be filled, from its chief source in the Ben Nevis group. But it seems impossible that such masses of ice could deposit a moraine in the situation required; and it even appears doubtful whether sheets of ice would deposit moraines at all. On these grounds I do not see how we can admit Mr. Thomson's theory in its present form.

VI.—CONCLUSION.

I do not feel myself competent to express a decided opinion upon this "*vexed question*;" but regarding the lake theory as the true one, I think

it now only remains to be determined whether the barriers at the mouths of the glens consisted of ice, or of earthy materials. Perhaps we know nearly as much regarding the latter as we ever can know; but the valley of the Spean has never been carefully examined, with reference to the former passage of glaciers through it, by one fully competent to the task. Till this has been done, geologists are not, I think, in a position to decide between the rival theories.

A new white Gunpowder was exhibited, invented by the assayer of the mint at Constantinople, composed of sugar, chlorate of potash, and yellow prussiate of potash.

March 20, 1850.—*The* President *in the Chair.*

The following papers by Professor Thomas Graham of London, were presented by the author, viz.:—"On the Motion of Gases," Parts I. and II. "On the Diffusion of Liquids."—Thanks voted.

Mr. Bryce moved that the sum of £6 6s. be granted to purchase one or two Aneroid Barometers for the use of an association of naturalists, chiefly members of this Society, who were about to investigate the geology and natural history of the basin of the Clyde.—The vote was agreed to.

Mr. Stenhouse read a paper "On the Artifical Production of Organic Bases."

April 3, 1850.—Mr. Gourlie *in the Chair.*

The following were admitted members, viz.:—Messrs. Thomas R. Gardner, John Barclay, Robert Thomson, Thomas Neilson, Thomas Davidson.

Mr. Bryce's motion for a grant of £6 6s. for the purchase of an Aneroid Barometer, was submitted to the Society for the second time, and finally agreed to.

Dr. R. D. Thomson communicated the following paper:—

XIV.—*Composition of some Fermented Liquors. By* Mr. John Wright Currie.

The mode of conducting the experiments was to weigh out generally 2000 grains of the liquor under examination; it was distilled until the fluid passing over gave no smell of aldehyde, or any green colour with bichromate of potash and sulphuric acid. The distilled fluid, which consisted of alcohol and water, and the residue formed of saccharine and albuminous matter, were then weighed; the difference between the weight of the fluids and that of the original fluid was the loss. The specific gravity of the distilled fluid *was then taken*; then by referring to a table the per centage

of alcohol in the fluid was ascertained; then by multiplying the quantity of distilled fluid by the per cent. of alcohol in it, and dividing by the weight of the original fluid, the quantity of alcohol in the liquor is obtained. The following tables show the result of a few of the experiments conducted in this manner.

There is another method adopted by the excise, which consists in taking the specific gravity of the liquor under examination, distilling about ⅓ds. of it, then filling up to the original bulk with water, and taking the specific gravity. The second will be greater than the first, as the water is heavier than alcohol; by taking the difference between the two specific gravities, subtracting from 1000, and by referring to a low wine table, the quantity of alcohol in the liquor is obtained. Many of the experiments were checked by the second method.

With the exception of the Prestonpans beer, the wines and Dublin stout, all the specimens were manufactured at the Perth brewery. The Madeira was above forty years old and had been at Calcutta. The Sherry was upwards of twenty years old and had likewise been in India. The Port was about fifteen years old. The Samshoo is a spirit distilled by the Chinese from rice. It had been long in bottle, but was probably imperfectly stoppered.

TABLE I.

	FIRST EXPERIMENT.						SECOND EXPERIMENT.			
	Sp. Gra. by Saccharometer.	Quantity Distilled, in Grs.	Distilled Fluid.	Residual Fluid.	Loss.	Dried Residue	Distilled Fluid.	Residual Fluid.	Loss.	Dried Residue.
Prestonpans Beer,...	1011	2000	1476·4	478	45·6	70·2	1607	380	13	72·4
Small Beer,.....	1022	2000	1257	738	5	86·4	1567	400	33	85
Table Beer,.....	1005·5	2000	1415·6	543·4	41	55	1318	672	10	54
Common Porter,.....	1014	2000	1678	319	3	76·8	1738	251	11	77
Brown Stout,.....	1011	2000	1619	330	51	66	1656	340	4	65·4
Double Brown Stout,	1013	2000	1372	612	16	80	1525	455	20	80
Imperial,.....	1024	2000	1725	275	0	110	1560	440	0	110
Export,.....	1014	2000	1320	640	40	74	1552	430	18	76
India,.....	1005	2000	1872	128	0	55	1294	660	46	55
No. 3 Ale,.....	1026	2000	1371	570	59	114	1385	605	10	114
No. 4 Ale,.....	1039	2000	1384	610	6	133·4	1254	706	40	133·6
Port Wine,.....	...	2000	1382	600	18	47	...	...	...	...
Madeira, 40 yrs. old,	·9888	1840	1304	516	20	73	1360	610	30	79
Sherry,.....	1002	1680	1570	90	20	60	...	...	...	...
Porter—Guiness's Dublin,.....	1016·5	2000	1226	760	14	100	...	...	...	...
Claret,.....	·9958	2000	1630	356	14	74	...	...	...	...

TABLE II.

	FIRST EXPERIMENT.					SECOND EXPERIMENT.				
	Spec. Grav. of Fluids.	Per Cent. of Alcohol in Fluid.	Total Alcohol in grs. in Liquor.	Per Cent. of Alcohol in Liquor.	Per Cent. of Dry Residue	Spec. Grav. of Fluid.	Per Cent. of Alcohol in Fluid.	Total Alcohol in Fluid.	Per Cent. of Alcohol in Liquor.	Per Cent. of Dry Residue
Prestonpans Beer,	·9928	4·	59·056	2·95	3·51	·9954	2·5	40·175	2·10	3·62
Small Beer,	·9963	2·	25·14	1·25	4·32	·9954	2·5	39·175	1·95	4·25
Table Beer,	·9919	4·6	65·117	3·25	2·75	·9921	4·4	57·992	2·89	2·7
Common Porter,	·9918	4·7	79·866	3·90	3·84	·9918	4·7	81·906	4·08	3·85
Brown Stout,	·9902	5·6	90·664	4·53	3·3	·9902	5·6	92·736	4·63	3·27
Double Brown Stout,	·9858	8·6	117·992	5·89	4·	·9858	8·6	131·15	6·55	4·
Imperial,	·9869	8·	138·0	6·96	5·5	·9846	9·4	145·64	7·28	5·5
Export Ales,	·9807	12·4	167·68	8·36	3·7	·9842	9·75	151·32	7·56	3·8
India Ale,	·9837	10·1	189·07	9·4	2·75	·9797	13·2	170·80	8·54	2·75
No. 3 Ale,	·9836	10·25	140·52	7·02	5·7	·9836	10·25	141·96	7·09	5·7
No. 4 Ale,	·9817	11·6	160·54	8·02	6·67	·9807	12·4	155·49	7·77	6·68
Port Wine,	·9668	23·6	326·15	16·3	2·35	...	...	...	...	...
Madeira,	...	...	...	4·75	3·96	·9745	17·4	236·44	11·832	3·95
Sherry,	·9777	14·9	233·93	14·09	3·57	...	...	...	...	...
Guiness's Dublin,	·9858	8·6	105·43	5·27	5·	...	...	...	...	...
Claret,	·9838	10·	163·0	8·15	3·85	...	...	...	...	...

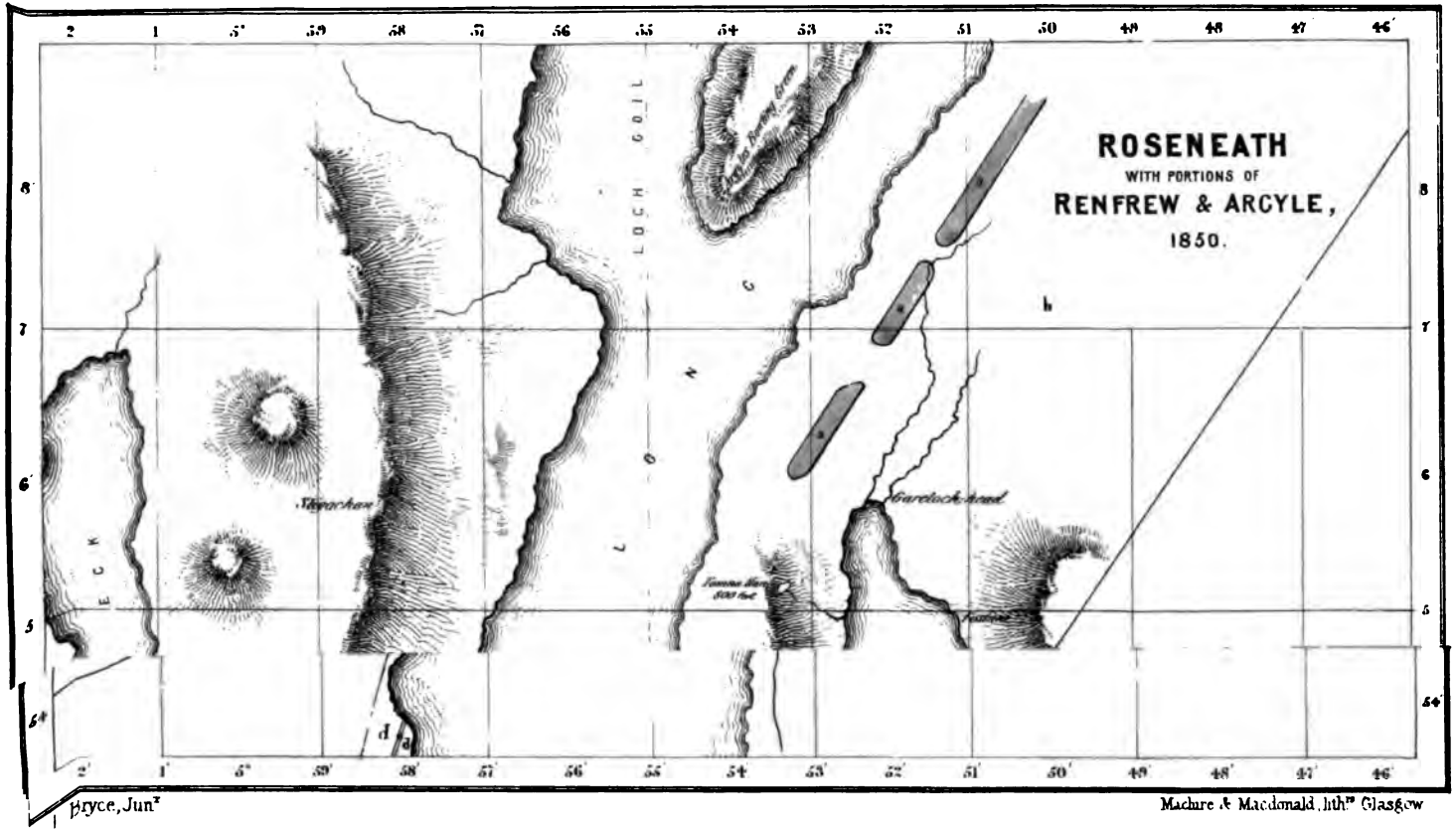
TABLE III.

	Mean per cent. of Alcohol in Liquor.	Mean per cent. of Dry Residue.	Per cent. Water		Mean per cent. of Alcohol in Liquors.	Mean per cent of Dry Residue.	Per cent. Water.
Prestonpans Beer,	2·525	3·565	93·91	India Ale,	8·97	2·75	89·28
Small Beer,	1·60	4·285	94·12	No. 3 Ale,	7·055	5·7	87·25
Table Beer,	3·07	2·725	94·21	No. 4 Ale,	7·855	6·675	85·47
Common Porter,	4·035	3·845	92·13	Port Wine,	16·3	2·35	81·35
Brown Stout,	4·58	3·285	92·24	Sherry,	14·09	3·57	82·44
Dble. Brown Stout,	6·22	4·	89·78	Madeira,	11.83	3·95	85·22
Imperial,	7·09	5·5	87·41	Claret,	8·15	3·85	88·
Export Ale,	7·96	3·75	88·29	Samshoo,	24·00	...	76·00

Mr. Alexander Mitchell brought before the Society an improvement which he has made in the construction and working of the Electric Telegraph. He began by describing the telegraph generally as consisting of three parts, namely, a battery to generate electricity, a wire to convey it, and a mechanical arrangement to communicate signals at once to different and distant places. The moving power in every case being the same, the difference in the various instruments in use consists solely in the mechanical combinations. Already sixty different machines have been invented, upwards of thirty of which have been patented. The objects aimed at in these diversified forms of the telegraph have been chiefly in the simplification of the arrangements, and the transmission of a greater number of signals at the same time. The desideratum still is, the transmission of intelligence by means *of one wire,* as rapidly and distinctly as it is now done by

three. All the different methods were classed under three varieties, namely the needle, the printing, and the step-by-step telegraphs. The signals by the first method are produced by the deflection of one or more needles; the second prints as well as transmits messages; and the third employs a revolving pointer to indicate letters or signs upon a dial. The telegraph commonly in use is the double-needled one, invented and patented by Cook & Wheatstone. The construction of this instrument having been described, and its operation illustrated by the transmission of messages betwixt two machines in different parts of the hall, Mr. Mitchell pointed out the principal defects of this method, as consisting in the expense caused by the necessity for having three wires, and in the liability of the wires to come into contact with each other. This is a contingency of such frequent occurrence as to require the constant employment of several men to separate the wires when the contact takes place. Another inconvenience arises from the difficulty of attaining perfect insulation of the wires. In wet weather it often happens that all the wires become connected electrically at the posts, by the water passing from one wire to another. What is still required, then, is an instrument combining cheapness with accuracy and rapidity. When this has been attained, the use of the electric telegraph will be brought within the reach of all classes of the community. At present the high charge for transmitting intelligence limits its usefulness for the purposes of trade and commerce. In America the telegraph is much more accessible. A message which would there be sent for several hundred miles, at the charge of half a dollar, would, in this country, cost nearly four dollars, or eight times more. In the United States there are about 9000 miles of telegraph; in England between 2000 and 3000; in Scotland only about 160 miles. In America, the expense of fitting up a mile of telegraph is about £30, and being generally single wires, the expense of maintaining them is inconsiderable. In this country the price per mile is about £150, and the expense of maintenance proportionably high.

The plan invented by Mr. Mitchell, and which was illustrated by a machine in working order, consists of only one wire. Instead of the arbitrary and conventional symbols of the ordinary method, which are deficient in precision, and liable to misapprehension, Mr. Mitchell employs the ordinary Roman Alphabet and numerals, along with the common figures. The letters are painted upon a segment in front of the operator, and corresponding letters are inscribed on four-and-twenty ivory keys, placed exactly as in the piano-forte, and touched in the same manner. The letters are so distinct that a child could scarcely err either in communicating or receiving a message by the instrument. When one of the keys is pressed down, a needle instantly points to the corresponding letter on the segment; and when a series of instruments are established at the different stations along a line of railway, by the simple act of pressing down the keys, one after another, the duplicate letters will be shown upon *all the instruments* in the same circuit. From 80 to 100 letters can be


ROSENEATH
WITH PORTIONS OF
RENFREW & ARGYLE,
1850.
LOCH GOIL
ECK
Garelochhead
Bryce, Junr
Machure & Macdonald, lithrs Glasgow

transmitted every minute, or as fast as they can be read, by means of Mr. Mitchell's invention. The working of the instrument was highly satisfactory. The ease, precision, and unerring accuracy with which it can be wrought, were made apparent to all present.

April 17th, 1850.—MR. LIDDELL *in the Chair.*

THE Society appointed Mr. Harvey, Mr. Gourlie, and Mr. Keddie to represent the Society at the Meeting of the British Association, to be held in Edinburgh in August next.

Mr. Bryce gave the following outline of the Geology of the Peninsula of Roseneath, and the adjoining tracts:—

XV.—*On the Geological Structure of the peninsula of Roseneath and the adjoining parts of Renfrew and Argyle.* By JAMES BRYCE, JUN., M.A., F.G.S.

I.—INTRODUCTION.

1. IN a paper which I had the honour to lay before the Society two years ago, on certain peculiarities in the geology of Bute, I promised, on a future occasion, to call the attention of the members to some analogous strata further up the Frith, which I had not then examined with sufficient care. They seemed to possess considerable interest; and on a more careful inspection of them I have not been disappointed. I propose to state briefly, in this communication, a few of the facts most worthy of being put on record.

II.—ROSENEATH.

a. Primary Strata.

2. The peninsula of Roseneath forms the south-western portion of the county of Dunbarton. It is bounded on the south by the frith of Clyde, and has the Gareloch and Loch Long on the east and west. It is eight miles long, and the breadth varies from one and a-half to two miles. It consists of a single irregular ridge of hilly ground, which runs through its whole length, scarcely reaching the height of 1000 feet, and declining with a nearly equal slope toward either estuary. In the northern part the rocks near the summit of the ridge rise into detached craggy knolls, presenting steep and bold fronts towards Loch Long; while the central and southern portions exhibit none of those rugged or serrated forms, which give a highly picturesque character to the adjoining tracts. But, although of such inferior elevation, and itself devoid of any remarkable feature, this ridge affords a series of more beautiful and striking prospects in every direction, than perhaps any part of the varied shores of our great western frith.

3. The geological structure of the peninsula is very simple. The upper and middle portions are composed of micaceous and clay slates, while old red sandstone occupies the southern part. These rocks are portions of the great bands of sedimentary strata which traverse Scotland from sea to sea, in a direction parallel to the principal axis of the Grampians, and to the great Caledonian valley. They deviate very little from their ordinary type, and it is therefore unnecessary to enter into any lengthened descriptions. The usual varieties, depending upon the varying proportions of the constituent minerals, occur abundantly, but in no definite order, and without much continuity. Thus the clay-slate series exhibits beds of flinty slate often approaching to quartz rock, of highly bituminous slate, and of coarse grained compact thick bedded slate, mixed up irregularly with the commoner kinds, such as coarse and fine roofing slates and a semi-crystalline silky slate, passing into chlorite or talcose schist. All these varieties are well seen on the road-side between Roseneath and Kilcreggan, and on the shore between the latter place and North Ailey. On the same coast thick cotemporaneous beds and also veins of quartz occur, in the cavities of which rock-crystal is often met with. Roofing slate of good quality is obtained from several quarries; but neither in this rock nor in the mica slate which underlies it, have any indications of metallic ores been noticed. Iron pyrites occurs in the slate rocks in many places. Beds of quartz containing rock-crystal occur frequently in the mica slate, and are well seen on the road-side to the west of the village of Gareloch-head. To the east of Tom-na-hary hill, I found crystals of schorl in a variety of mica slate, containing very little quartz.

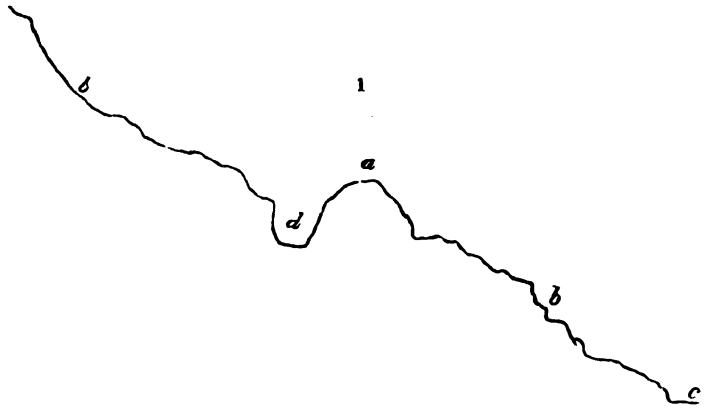
4. In the accompanying map, these two slate rocks are marked as separated by a definite boundary; but in nature no such distinction exists; the transition is in fact so gradual, that it is impossible to say where the micaceous series terminates, and the argillaceous commences. Towards its outer boundary the mica slate begins to assume the character of chlorite schist, and to contain occasional beds of fine roofing slate, the true mica slate still constituting the greater part of the mass. Farther out, the argillaceous and chlorite slates begin to prevail, so that the micaceous beds may be said to be subordinate to them; and thus through oft repeated alternations, we at last reach the true clay slate series. We can conceive, therefore, of a certain middle line, along which the strata partake equally of both characters; it is this imaginary line which in such a case may fairly represent the boundary. These remarks are equally applicable to the other slate rocks of the district, and indeed to all the rocks of this class in the West of Scotland. In order to explain the mode in which this gradual loss of a marked character, and assumption of one considerably different was brought about, it is necessary to remember that the slate beds were originally deposited from the sea, layer over layer, in the state of silt or fine mud, and afterwards exposed to great heat, combined with pressure. A slight change in

the nature of the sediment, or in the amount of heat or pressure, would be sufficient to produce the want of uniformity, and the variations from a definite type, which we now observe. The general direction of the beds is about north-east and south-west, the dip being to the south-east, at angles varying from 40° to 70°;—but there are many local exceptions.

b. Plutonic and Metamorphic Rocks.

5. The slates above described contain subordinate beds and veins, which possess considerable interest.

In passing over the summit level of the road between Gareloch-head and Portincaple Ferry, the observer who is accustomed to distinguish rocks from a distance, by the peculiar forms which the different species impress upon the surface, cannot fail to notice a remarkable ridge on the right, projecting from the smooth outline of the hill side, and trending in a straight line towards the base of the mountains. Its form, as seen from the road, is represented in the annexed sketch, No. 1, which gives the



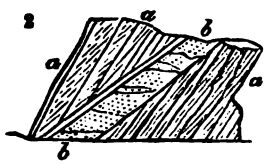
a Ridge of porphyry, rising through the mica slate.
b b The hill sides formed of mica slate.
c Summit level of the road between Gareloch and Portincaple.
d Mountain stream running along the junction.

outline of the surface. It is obviously composed of matter erupted through the slate, and on examination is found to consist of a highly felspathic rock. The base of this rock is a very compact mixture of quartz and felspar, in which crystals of felspar and mica are imbedded, the latter ingredient being constantly present, the former often wanting. The prevailing colour is yellowish red, given by the felspar in the base. From the prevalence of felspar, and the mode in which the crystals are disseminated, the rock must be called, according to the present views, a felspar porphyry; though the constant presence of mica as a constituent, and the compound character of the base, seem rather to require that it should be considered a granite.

At the sides of the vein next the slate, the rock is of a more homoge-

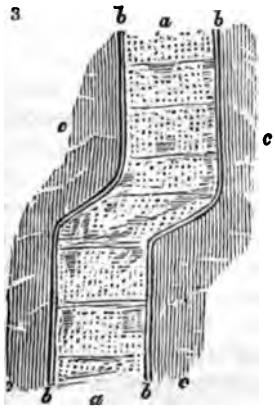
neous character, resembling a compact claystone or flinty slate, but still enclosing crystals of mica. The laminæ of the slate in contact with the sides of the vein have a very close resemblance to this variety; being, in fact, a yellowish grey fine grained flinty slate, with occasional spangles of mica. It is thus difficult to determine the exact boundary line between the slate and the vein. This assimilation of mineral character has obviously been induced by the cooling of the masses from a state of fusion, at nearly the same rate, but more rapidly than the inner portions of the vein. The breadth of the vein is various; in some places no more than twelve or fifteen yards, in others as much as twenty-five, and even thirty, and perhaps considerably above that, as it cannot in many cases be exactly measured. It rises above the general surface of the hill side from fifteen to forty feet in different places.

6. The course which this erupted mass follows is indicated on the accompanying map. In the northern part of its course it is interposed as a bed between the strata of slate. To the south of the high road, on the side of a little stream, it is seen intersecting the beds at a small angle, as in the annexed sketch, No. 2, which is a *vertical section*. A little farther south the ground rises considerably, and the vein does not appear upon the surface; the mica slate occupying the entire space where it is possible the vein could appear; but on the lowering of the ground still farther south, it again emerges from beneath the slate and occupies the surface for some distance. A similar overlapping occurs farther north, near the high road. Both of these are expressed on the map.



a a Mica slate, highly inclined.
b b Plutonic rock intersecting the beds.

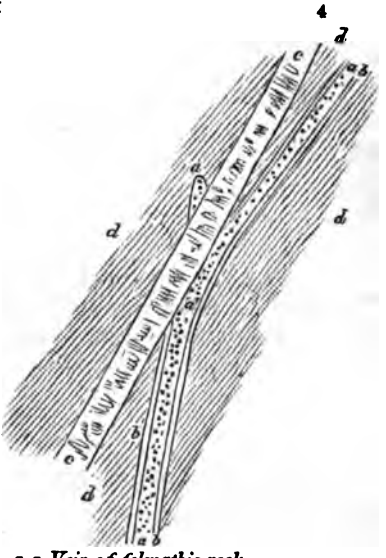
Though preserving a general N.N.E. and S.S.W. direction this vein makes several undulations, throwing it a good deal out of its usual course. The most remarkable of these is seen not far from its northern termination, and is represented in the annexed sketch, No. 3, which is a *ground plan*.



7. The appearance of this plutonic rock, now as a bed interposed among the strata, and again as a vein intersecting them, and the undulating course which it pursues, point out its posterior origin and the nature of the resisting force. This is plainly to be found in the peculiar undulations and twisted forms which everywhere characterise the mica slate, indicating the powerful compressing forces which acted upon it while yet plastic under the influence of heat.

a a Vein of felspathic rock, about 25 yards wide. *b b Metamorphic schist.*
c c Common schist in beds nearly vertical and parallel to the vein.

A little way north of this point, and at another bend in the vein, the outer or salient angle is intersected by a dike of greenstone and basalt, in such a manner that a portion of the felspathic rock is isolated between the whin dike and the mica slate, and the continuation of the vein lies on the same side of the dike as before the intersection. The annexed sketch, also a *ground plan*, shows the mode of this intersection, which is the most singular I have ever met with.



a a Vein of felspathic rock.
b b Altered schist.
c c Dike of greenstone and basalt.
d d Mica slate.

The dike is very distinctly traceable for several hundred yards towards the north-east, the surface occupied by it rising into conical hummocks. It is then lost in marshy ground for a short distance, but is again continued towards the mountains. In the other direction, after a lengthened and most careful search, its course was satisfactorily made out as far as Portincaple Ferry, where it is well seen; and I have met with a dike of the same width and bearing, near the top of the mountain on the east side of Loch Eck, traversing mica slate and altering it considerably; which, I have no doubt, is a prolongation of this dike. The width is about 25 yards, and it bears a point S. of W. It is in many places inclined at the same angle as the slate, among the beds of which it seems to have insinuated itself in a serpentine course. The mica slate in some places is slightly changed by the contact, being rendered harder and more massive: the lamination is partially destroyed, and the rock is banded, parallel to the sides of the dike. In one place pieces of the slate are seen enclosed in the dike, and slightly altered. Portions of the wedge shaped mass of slate, *d*, between the two veins, are entangled in the basaltic dike, and altered in the same manner.

At the edges the dike consists of blue slaty basalt, but the greater part of the mass is a coarse grained greenstone, which at several points exhibits in great perfection that peculiar structural arrangement in concretionary spheroids, which is the most frequent and characteristic form assumed by the trap rocks, and of which the columnar is but the result, when under favourable conditions, they parted more slowly with their heat of fluidity. The best marked of these is in a cliff about 60 feet high, overhanging the marshy ground above mentioned, where the dike has a considerable underlie, the slate being in contact on both sides.

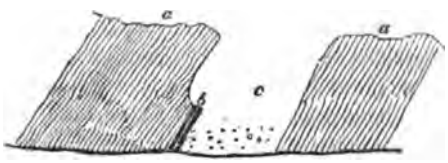
It is here divided into *columns of spheroids* perpendicular to the sides of the dike, and separated from one another by imperfect joints. Sometimes each joint is composed of a single spheroid; one was noticed measuring 15 inches by 10; in other cases numerous small closely packed spheroids make up a joint. Instead of a distinct separation as in basaltic pillars, the columns are connected by narrow seams of decomposed greenstone. The columnar structure is here seen in the act of development—if the heat had parted more gradually, a façade of pillars would have been the result. See the remarkable experiments of Mr. Gregory Watt on fused basalt, (Phil. Trans. 1804) of which this spot affords an excellent illustration.

It will appear from the foregoing statements, that the small area we have been describing, is one of considerable interest, exhibiting, as it does, the rare association of many species of erupted rocks in connection with the primary strata; and affording illustration of some curious questions in theoretical geology.

c. Limestone.

8. At a place called the Cove, in the townland of North Ailey, on the shore of Loch Long, a bed of limestone is interstratified with the clay slate. It has been originally six or seven yards wide, and has extended eastwards across the low ground between the shore and the cliff, into the cliff itself, and probably much further inland; but it cannot be satisfactorily traced. The part next the shore has been almost entirely removed by quarrying; but from portions which are found among the slate—as in the annexed sketch, No. 5, a *vertical section*—there can be no doubt of the true position of the bed.

5



a a Inclined strata of clay slate. b Bed of limestone. c Bay with accumulations of shingle.

The limestone is impure, from intermixture with slaty laminae; the prevailing colour is bluish gray; it contains much calcareous spar; and, like the slate, is destitute of fossils.

In the new statistical account of the parish of Row adjoining, beds of limestone are stated to occur in the slate rocks of Glenfruin; these are most probably similar to the bed now mentioned.

d. Old Red Sandstone.

9. *The junction* of the slate and sandstone is strikingly marked on

the features of the landscape.* It crosses nearly through the middle of the remarkable and very picturesque dell, which intersects the peninsula from Campsail Bay to Kilcreggan in a direction nearly north-east and south-west. Thus, in external aspect, and in the nature of its rocks, this southern portion is isolated from the rest; it consists of a single hill of a depressed conical form, having a smooth outline, and extending in gentle and fertile slopes to the water's edge on three sides. Here, as in other places, the soil formed by the decomposition of the sandstone contrasts most favourably with that which rests upon the cold retentive clays of the coal formation on the one side, and the old slate rocks on the other. The series exhibits but its lowest members—conglomerates, coarse sandstone, and finely laminated red sand, irregularly disposed. The base of the conglomerate is coarse red sand; and the imbedded fragments are granite, porphyry, quartz, and various kinds of slate; the three former are very much rounded, the latter have lost their angularity, and present elliptic forms. The origin of these is to be looked for in some near district of the Grampians, where such varieties exist, and which we know were elevated and exposed to the action of mechanical forces prior to the epoch of the old red sandstone; the quartz and slate pebbles are from the adjoining strata.

The contact of the sandstone and slate is nowhere seen. In the western part of the cliffs at Portkill Bay, the two rocks approach very close, the sandstone dipping at a small angle towards the nearly vertical slate; and in Campsail Bay a considerable space of flat beach intervenes, concealing the contact. The line of junction passes near the Saw-mill, and across the upper part of the fields, which slope down from the northern edge of the plantation crowning the heights on the south side of the great hollow or dingle.

On the south coast of the peninsula the sandstone dips for a short distance towards the south; the dip then changes, and continues in other parts to be between west and south-west, at an angle of about 15° to 20°.

e. Proofs of Elevation.

10. In the paper referred to in Art. 1. the phenomena which indicate a change of level in the sea on the shores of Bute were so fully stated that it is unnecessary to enter here at any length into the subject, the evidences being of the same character. A well defined terrace exists along most parts of the shores of the peninsula. It is but faintly impressed upon the bold rocky shores (Art. 3) in the northern part. But from Culeport southwards to Kilcreggan it is extremely well marked. The breadth varies from ten or twenty yards, to one hundred, or even more. It is terminated inland by a perpendicular cliff, twenty to forty feet high, composed of slate rocks much worn and often hollowed into caves. In some

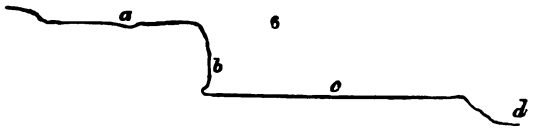
* "Rosneath" is said by some to mean in the Gælic language, "the little dell or dingle;" and that the name was given to the whole from this peculiar feature. See New Statistical Account under "Roseneath parish, Dumbartonshire."

places high tides rise to the very edge of the terrace, but in general it is from ten to twenty feet above the level of the tide; and the cliffs show evidences of the action of the waves to near their summits. On the surface of the terrace, huge masses of slate rest in some places, being portions of the original rock, which, from their extreme hardness and toughness, have longer resisted, but bear obvious marks of the action of the sea. The largest of these seems to be several hundred tons in weight: it is situated in the townland of Ailey, and is known over the whole peninsula as the "Big Stane." The coast road is carried along this terrace, and upon it here, as on all the shores of the Frith, the coast residences are erected. Owing to its flatness and the high cliff behind, it is often swampy and difficult to drain, and contains deposits of peat with trees. In sinking for foundations it is very common to find sand or clay mixed with sea-worn stones and sea-weed.

In passing eastward from Kilcreggan and entering on the sandstone district, the terrace is found to expand very much. It is from 200 to 400 yards wide in most places, and in some much more, as in the eastern part, near Roseneath castle, where it stretches out into a low, flat promontory fully half a-mile in extent. Contracting northwards from this for some distance, it again expands between the "Clachan" and the landing pier, into a wide level tract, the outer part of which is a mass of fine shingle cast up by the sea. In the western part, near Portkill, the surface of the terrace consists of round sea-worn stones imbedded in the scanty soil. These are all masses of primary rocks; and the immediate shore, which is here composed of sandstone, is strewed thickly with the same. They consist of mica and clay slates, granite, quartz, and porphyry, all of which exist only to the north-west of this point, and whose transport hither must be referred to the long continued action of tides and currents and prevailing winds. One mass of mica slate, whose parent rock is at least two miles distant, seems too large to have been thus transported, being upwards of twelve feet in each dimension. Eastward these loose masses diminish, and the terrace to the east and south-east of the castle consists chiefly of sand or sandstone. At Portkill a bold precipice of conglomerate rises behind the terrace to the height of nearly forty feet, and presents the most unequivocal evidence of the former action of the sea. It is undermined in many places, and overhangs its base; veins of red sand are washed away, and beds of hard conglomerate project; it is cut into grotesque forms, and hollowed out into considerable caves, which are large enough to afford residences to families of gypsies, or troops of mussel-gatherers, for weeks together.

These cliffs are succeeded towards the east by steep grassy slopes, marking the boundary of the terrace, and terminating in rich fields descending to the edge of the water; the whole presenting, with the tasteful plantations, a most pleasing scene of pastoral beauty. To the south-west and west of Roseneath house the cliff is resumed. Here, at and on both sides of a spot called "Wallace's loup," the same evidences

of the action of the sea, as have been just mentioned in reference to Portkill, are to be seen in the perpendicular cliffs of sandstone, which run nearly east and west for about a quarter of a mile along the south side of the principal approach, and then turn south at a right angle, and gradually subside towards the slopes above-mentioned.



a Upper-slopes on which the offices stand, perhaps a former beach.
b Sea-worn cliff of sandstone, called Wallace's loup.
c Terrace, or former beach, on which Roseneath house stands.
d Sea level.

11. Through the kindness of Lorne Campbell, Esq., of Roseneath, I am enabled to present a valuable series of levellings of these terraces, which has been made under his directions, and which it is very desirable to have compared with the heights of the other terraces in the Frith, viz.:—

1. Height of the base of the north portico of Roseneath house above high water, - - - - - - - - 42 feet.
2. Base of "Wallace's loup" at the south end, level with the above; face of rock 12 feet high, top sloping steeply to the west, 42 + 12, - - - - - - - - 54 —
3. Lower part of the base of this above high water, 26 feet, height of rock at this point, 33 feet, - - - 59 —
4. Height of the stables, &c. above high water, - - 79 —
5. Height of a hollow adjoining these buildings, where shells, sea-weed, &c., were found some years ago, 5 or 6 feet below the surface, - - - - - - - 68
6. Height of the base of the cliffs at Portkill, at the north end 38 feet; rock, 18½ feet, - - - - - - 56½ —
7. Lower part of the base of the rock, 25 feet; height of the rock, 37 feet, - - - - - - - - - 62 —

It thus appears that there are indications of terraces and former beaches at still higher levels; and that the one we are now more especially considering, which marks the last upheaval, preserves a pretty equable level round the peninsula, and corresponds with the terraces in Bute.

I am informed by James Smith, Esq., of Jordanhill, that when the foundation was dug for Roseneath house, shells and sea-weed were found mixed with shingle; and that this shingle consisted chiefly of flat pieces of slate about the size of a penny; whereas on the present beach the fragments are mostly round. He considers that the whole terrace at this part consists of such marine shingle; by levelling, the same height was obtained for this beach as that above given. The discovery of these

beds and of similar shelly deposits on the opposite coast at Ardencaple, first noticed by the late Duke of Argyle, led Mr. Smith to that extensive and most valuable series of investigations by which he established the former existence of a sub-arctic climate, and a distinct place for the Clyde beds among the tertiary strata.

Within the Gareloch the cliff bounding the ancient beach is not marked with the same continuity or distinctness; but where it is seen, the levels appear to be the same as those already mentioned.

POSTCRIPT, AUGUST, 1850.

f. Scratched Rocks.

12. The striated and grooved rocks of Gareloch have been described by Mr. Charles M'Laren in several papers in late numbers of Jameson's Journal; and it is therefore unnecessary to enter into any details respecting them. I have verified his observations in several places, and have no doubt that the phenomena are described with that precision and strict attention to facts which mark Mr. M'Laren's other productions. But I cannot agree with his conclusion, that they are due to glacial action. It seems much more reasonable to connect them with the deposit of the boulder clay, and the transport over the plains of Lanerkshire of rocks which exist only to the north-west, and which are often striated and polished. The total absence of any such gravel deposits as could be considered moraines is a serious objection; for though Mr. M'Laren gives one instance in the shingle-bed at Row-ferry, I think it is perfectly clear, from the sea-worn character of its materials, that this is a true bank of shingle, formed by the sea in consequence of the peculiar movements of the tide at this part. Owing to the narrowness of the outlet, and the great extent of water inside, the tide ebbs at Row-ferry with a powerful current: between this and the surf the bank has been thrown up, there being both at flood and ebb a stream on the western side of the outlet, and an eddy on the eastern. This fact has been pointed out to me by Mr. Smith, who also states that the shingle bank *rests upon* the old boulder clay or supposed glacial deposit, and must therefore be of later formation than the rock-striation and boulder-transport. Other weighty objections are ably stated in Professor Oldham's anniversary address of this year to the Dublin Geological Society; and the subject need not therefore be pursued farther. The most accessible points for seeing the striation and grooving are at the landing place at Row; at the point of divergence of the roads leading along the east and west sides of the loch, about a quarter of a mile above Gareloch-head; and at several points on the road-side, between the latter place and the summit level, particularly on the east side of the road, exactly at the summit. But at none of the points which I visited were any marked examples of polishing noticed, or of that peculiar "moutonnée" character which is so extremely well defined in the case of rocks in the lake district *of Westmoreland*, which I have lately described, and of some pointed out

to me a few weeks ago by Mr. Robert Chambers, at Parson's Green near Edinburgh. They resemble more the cases long ago described by Sir James Hall, as occurring at Corstorphine hill and several other places in the neighbourhood.

III.—EAST COAST OF THE COWAL DISTRICT.

13. This coast consists of slates of the argillaceous and chloritic series, passing westwards into mica slate. In the southern part, between Innellan and Toward Point, old red sandstone, with its associated cornstone, occupies the coast, extending, however, no farther inland than the terrace, into the landward boundary or wall of which the slate rises to a considerable height; but in neither the sandstone nor limestone have fossils yet been found. The old slates near Dunoon are associated with rocks of igneous origin, to whose effect upon the slates, and their own peculiar forms, much of the picturesque beauty of this favourite watering-place is due. Thus the ridge lying between the coast and the valley of Hafton lake, owes its elevation and bold outline to an outburst of igneous rocks, which have induced a very decided change upon the slate along the planes of contact. It consists of crystalline greenstone, of a different type from that of the dikes common on the coast, the structure being slaty and the hornblende in excess. It is from 60 to 100 yards wide, and ranges from near Hunter's Quay, across the highest part of the ridge, transversely to its length, appearing along the summit in a series of conical hummocks, with deep hollows between; and thus presenting a bold picturesque outline when viewed from the low grounds in the neighbourhood. It is interrupted by the Hafton valley, but is resumed on its western side, and attains its greatest altitude in Dunloskin hill, which rises prominently above the surrounding slopes, strikingly relieving their monotonous outline. Westwards, for about half a-mile, it is seen in other rocky eminences, but its farther extension in this direction was not traced. The ridge is not seen intersecting the coast, which is everywhere occupied by the slate rocks; so that it seems to terminate before reaching the shore. Owing to the metamorphic character which has been impressed upon the adjoining slaty beds, it is difficult to determine the precise limits of the plutonic rock; near the contact the slate breaks under the hammer into very compact four-sided prisms.

In a similar manner, the high ground dividing the East and West Bays, and projecting beyond the general line of coast, has acquired its strikingly picturesque aspect from a great dike of basalt which traverses it. The castle hill consists of this dike, and of slate borne up with it, and adhering to it. By contact with the dike, the slaty structure is effaced; the rock has been fused and reconsolidated into a compact flinty slate, closely resembling basalt; crystals are developed along the boundary, and bands of different colours are disposed parallel to the sides *of the dike.* The width is about 100 feet, and the bearing

W.N.W. The Gantock rocks are exactly in the line of bearing, but were found to consist of very hard slate. On the opposite coast, however, near Ardgowan, a dike of the same width and direction occurs, which may be the continuation.

IV.—COAST OF RENFREWSHIRE.

14. The coast section of this county presents only old red sandstone and trap, with occasional beds of limestone. The sandstone rises seaward, dipping east and south-east, at a small angle, and everywhere occupies the coast except at Kempoch Point and Cloch Point, where the overlying trap reaches the coast line, and is seen between high and low water, resting upon and altering the sandstone. The great overlying mass of trap sends out innumerable dikes intersecting the sandstone of the coast, and running in very various directions, but with a general tendency to the west and north-west. Into a general description of these, and the changes produced by them upon the rocks which they intersect, we cannot enter in this place. One instance only will be given, as bearing upon the subject referred to in Art. 1., in the changes induced upon the limestone at Innerkip. This rock appears in two places near Innerkip; one bed extends from the bridge on the Greenock road, at the north end of the village, up into the hill on the south-east of the village, rising with the slope of the sandstone beds, and preserving a thickness throughout, of about 12 feet; it has been extensively quarried behind the village, but is now little worked. As it is extremely hard, and contains much chert disseminated in veins and bands, the rock is capable of taking a fine polish, and being applied to ornamental purposes;—the colour is dark grey. Between the limestone and the sandstone above it, there is interposed a bed of loose materials, consisting of red sand, spotted with round grey spots, and enclosing pieces of limestone and sandstone. Hence the bed of lime must have been exposed to considerable decomposition before the sandstone was deposited over it. A lengthened and careful search was not rewarded by the discovery of any fossils. It is however obviously in the position of the cornstones of England.

Several beds separated by strata of sandstone occur on the shore at the mouth of the river. The whole series is here traversed by dikes of greenstone, the largest of which is about sixty feet wide, and ranges about N.N.W.; the others are so numerous, and so ramified, as almost to defy description. They pierce through the limestone in every direction, thin veins branching from the greater, and often again uniting, while small portions of the limestone and sandstone are entangled in the trap, and traverse it in disconnected veins. The changes produced upon the limestone are of the most interesting kind; I know no locality in the West of Scotland where the posterior origin and intrusive character of the trap rocks are so clearly manifested, and I would strongly recommend it as a point to be visited by the student *of geology.* The changes which the limestone has undergone run through



every variety of external aspect, from the impure, dark coloured, perfectly opaque state, to that of a pure white marble, translucent on the edges, homogeneous throughout, and devoid of stratification, or visible lines of cleavage. Intermediate between these extremes there are an indurated semi-crystalline limestone, a granular saccharine marble crumbling into fine powder under slight pressure, and phosphorescing when thrown upon a heated surface; a very hard white or blue crystalline marble, having the crystals in distinct plates, besides other gradations, similar to those described in the paper on Bute already referred to, the degree of alteration depending on the distance from the sides of the dike. The entangled portions are among those which exhibit the greatest amount of change. The most altered parts of the sandstone resemble quartz rock.

15. In order to determine whether any and what chemical changes had been induced simultaneously with these alterations of mineral character, and to afford terms of comparison with the metamorphic action upon limestones of the same age in Bute, (Art. 1,) Dr. Robert Dundas Thomson has most kindly furnished me with analyses of several specimens, made under his care in the laboratory of the University. These are as follows:—

Specimen No. 1, is the unaltered limestone, as pure a specimen to the eye as could be selected.

No. 2, is the saccharine marble, crumbling readily into powder, in the same state as No. 1 of the Bute specimens. (Proc. Phil. Soc. Glas. vol. III. page 20).

No. 3, is the most altered specimen in this locality, described above as "a pure white marble, translucent on the edges," &c., and which turns out to be Table spar.

No. 4, is a carefully picked specimen of the same, free from carbonate of lime.

No. 5, is a calcareous sandstone, altered by contact, scarcely distinguishable from No. 2.

No. I.

By Mr. P. Kater.

Carbonic Acid,	42·40
Lime,	54·58
Silica,	0·70
Magnesia,	0·40
Water and loss,	1·92
	100·00

No II.

Carbonic Acid,	42·52
Lime,	54·68
Insoluble Siliceous Matter,	1·14
Magnesia,	a trace.
	98·34

No. III.—Spec. Grav. 2·88.

	By Mr. T. Carlile.		By Mr. R. Kirkwood.	
Silica,	47·50	48·00	46·64	49·38
Lime,	44·90	45·40	46·27	44·59
Protox. of iron and alumina,	5·40	1·61 2·01	1·44	1·49
Water,	0·80	0·80	0·80	1·60
Magnesia,	0·70	0·83		1·05
Soda,		0·92		·94
Carbonic acid and loss,			4·85	·95
	99·30	99·57	100·00	100·

No. IV.

By Mr. R. Kirkwood.

Silica,	49·380	In atoms this is	
Lime,	44·596	Silica,	24·89 1.93
Alumina and protox. of iron,	1·490	Lime,	12·74 1
Soda,	·936	And the formula is, omitting the impurities—	
Magnesia,	1·050	CaO,2SiO,	
Water,	1·600	or Bisilicate of Lime.	
	99·052		

No V.

Carbonate of Lime,	31·94
Insoluble Siliceous Matter,	68·06
	100·00

It appears from these analyses, 1. That the cornstones of Innerkip are carbonates of lime, and not dolomites, as are those of Bute.

2. That in this locality igneous action has converted a carbonate of lime into a bisilicate. Now, as it appears from Analyses 1 and 2 that there is but a trace of siliceous matter in the limestone, the origin of the silica must be sought in the igneous rock; in fact, a transference of a portion of its silica must have taken place when the basalt was in a state of fusion. Such transfer, indeed, could readily take place under the influence of chemical attractions, when the rocks were in a state of even imperfect fusion. *See the Paper already referred to*, in Art. 1, *sub finem*. On No. 4, or the Table spar, Dr. R. D. Thomson remarks:—

"It is an interesting fact in connection with this mineral, that it gives *a yellow* colour before the blowpipe when moistened with muriatic acid;

and yields crystals of common salt when treated with the same acid. It is thus associated with Wollastonite, or soda-table spar, a mineral occurring in the Bishopton tunnel, and in the Kilpatrick hills." There is however this difference between the two cases, that in the localities last mentioned the mineral has no direct connection with limestone, although calcareous spar occurs abundantly; whereas at Innerkip, it has obviously originated in a change induced upon common limestone.

The following communication was made:—

XVI.—*Observations and Experiments on the Paper Manufacture, with some Improvements on the usual process.* By JOHN MACADAM, Lecturer on Chemistry.

THE author first gave a condensed statement of the process of paper making, especially of those varieties used as printing papers, and which are rosin-sized. His descriptions were illustrated by suitable diagrams, specimens and experiments. Mr. Macadam then referred to various applications of Chemistry to this important branch of manufacture, and certain improvements he had effected on the customary process. The processes and improvements detailed had reference to the paper factories of Messrs. Alexander Duncan & Son, of the Herbertshire Mill, Denny, and Messrs. R. & J. Couper of Cathcart; and for obvious reasons are here referred to, in a partial and abridged form.

Under the chemical part of the subject, the soda-ash used in the cleansing of the crude rag was first treated of. Soda-ash appears to have been employed in paper works in a partially or totally undecarbonated state. The alkaline solutions in use were therefore far from being economically prepared. Caustic soda ley had been found more effectual and economical. The proportions recommended for its preparation were 121, lbs. of soda-ash, containing 44 to 48 per cent. of alkali, 100 lbs. of best caustic lime, and 150 gallons of water. The proportion of water stated is sufficient, if maintained throughout the preparing process, to prevent the after decomposition of carbonate of lime by concentrated leys, noticed by Liebig. The proportion of lime is higher than the theoretical quantity, but was found in practice to yield a thoroughly caustic ley when waste steam was the heating agent, a source of heat very generally at command. The usual precautions adopted in laboratories, as the boiling of the ash in water, with the addition of the lime in the state of a creamy paste, in successive portions—the boiling of the soda solution between each addition of lime—the supplying of the loss of water by evaporation, and the covering up of the material when made, were enforced. Some apprehension existed, that the soda, in its caustic condition, might act too energetically on the rags, injuring the finer fabrics, and causing the stronger portions to become *ropy*, and consequently more difficult afterwards to reduce to the state of pulp. This was obviated by using the solutions at much lower

specific gravities than formerly, the rags being even then more thoroughly cleansed. In one of the paper works, the use of the caustic ley had thus effected a saving of 80 lbs. of soda-ash per day, or one-fourth of the material previously in use. As different strengths of leys are employed for the cleansing of the different kinds of rags, it was found that the possessing of a standard ley was advantageous. By this means a scale of measures had been adopted for charging the boilers for each variety. As the obtaining of a standard ley of a uniform strength, necessitated a constancy in the per centage of alkali in the soda-ash, the usual alkalimetry process was introduced, for the purpose of testing the ash on its arrival at the works. Mr. Macadam, before leaving this part of the subject, referred to two errors, not uncommon among manufacturers requiring caustic soda. First, The use of an aqueous solution of lime alone, obviously unaware of the slight solubility of that substance in water, and its decarbonating function; and Secondly, The using of the materials in the cold state, a practice very general amongst soap boilers, and indeed in some cases preferred by them. Although the digestion, under such circumstances, be a lengthened one, the maximum of causticity of the carbonated alkali is not obtained.

The solvent action of caustic leys on glass instruments was also adverted to. Chemists have long known that water exercises a very decided solvent action upon glass, especially when containing traces of Fluorine compounds, as has been lately shown by Dr. George Wilson, in reference to the corrosive action of some of the Edinburgh waters on glass instruments in use by brewers. As might be expected, alkaline solutions act much more powerfully, especially at high temperatures. From some observations made by Mr. Robert Clarke of the Campsie alum works and the author, glass hydrometers have been found to give way from this solvent action after a few months usage in hot and concentrated alkaline solutions; while others gave erroneous indications, to the extent of three or four degrees, because of the lightening of the instruments by the material removed from their surfaces. Mr. Clarke has instituted experiments to determine the rate of this solvent action, by weighing carefully a number of instruments before and after various periods of usage. Cold alkaline solutions act more slowly, but the action is still a marked one, and points to the necessity of devising some means of removing this source of error, in the determination of the strengths of leys. The coating of the instrument with gutta percha dissolved in chloroform might serve the purpose in regard to cold liquors, since the envelope adds little to its weight, and is transparent and unacted on by alkalies; but such an application is obviously useless in determining the specific gravity of solutions of elevated temperatures.

In reference to the bleaching process—bleaching powder, being variable in quality, being liable to adulteration and other causes of inferiority, a testing process is just as essential for a knowledge of its value, as in the *case of alkaline* compounds, for the determination of the pure alkali present

in them. The process recommended was that in which the peroxidation of the proto-sulphate of iron is made the index of strength. This process is best suited for commercial purposes, being easy of performance and speedy in result. It has long been a matter of difficulty with paper makers (as well as others) to find material of which to construct vessels capable of retaining solutions of bleaching powder for any length of time without leakage, as also to form the necessary piping, &c. for transmitting the liquor to the bleaching engine. This has been supplied in gutta percha. The receiving vessels are built of wood, lined with this substance. This application of gutta percha has been found not only in the highest degree effective, but consequently economical, the casements having been in use for fully twelve months, without being injured to any perceptible extent. The boots of the workmen in this department, and the smaller vessels employed are now manufactured from the same material. The hydrometer can be used with considerable accuracy in determining the value of bleaching liquors, when the original bleaching powder has been proved by experiment to be of good quality; but this instrument does not indicate the presence of impurity, and consequently the proportion of available chlorine. This is especially true in determining the value of partially exhausted liquors, or those which have been already in use. The hydrometer in this department was therefore superseded by the very satisfactory process of Mr. Walter Crum, as detailed in the printed proceedings of this Society. This process depends for its use upon the red colour of the per-acetate of iron, produced when solutions containing chlorine are brought in contact with the proto-acetate of iron; the depth of tint depending solely on the amount of available chlorine in the solution tested, irrespective of any generally occurring impurities which may be present. As the *waste* bleaching solutions from the pulp are collected, and form the basis of the contents of the bleaching engine, much foreign matters are unavoidably present in them; in short, the products of their previous bleaching action. The hydrometer cannot therefore be used as a test for the value of such partially spent liquors; thus, on testing a previously used solution of *bleach*, whose specific gravity was $1\frac{5}{3}$ by Twaddell's hydrometer, this liquor gave a lighter tint, with the stipulated proportion of "Crum's proof solution," than a fresh unused liquor at $\frac{1}{2}$ T. yielded. Another advantage attending the introduction of this testing process, was the capability of having a liquor of constant value in the cistern appropriated for the reception of the waste bleaching solutions, as well as a standard for fresh or unused ones. Moreover, as the varieties of pulp obtained from dissimilar kinds of rags require *bleach* of different strengths, the charging of the engine for each variety was effected with the greatest regularity, by this constancy in the value of the partially spent solutions, since the larger or smaller proportion of the standard fresh liquid was easily added to the contents of the engine, according to a scale of measures for each variety of rag. Phials indicating the tints *produced by the whole* mass of bleaching liquor, suited for each

kind of pulp, were also prepared and at hand, to serve as a check upon the contents of the engine, when charged according to the fixed scale of measures.

Mr. Macadam further referred to the late introduction of *Antichlore*, or sulphite of soda, for the purpose of removing any remaining traces of chlorine from the pulp, after being washed with water in the engine, subsequent to the bleaching process. The chemical action of this useful de-chloridising agent, under such circumstances, may be thus represented—

$$NaO\ SO_2 + Cl + HO = NaO\ SO_3 + HCl.$$

In reference to rosin-sizing,—The proportions which had been found most suitable for the preparation of rosin-size were 42 lbs. of crystallized carbonate of soda, 80 lbs. of rosin, and 60 gallons of water. The materials are heated in a water, in preference to a steam bath, and when dissolved are further diluted with 120 gallons of hot water. The solution thus obtained has a specific gravity of 1.014, and is strongly alkaline to test papers. Though slightly milky to appearance, the liquid is completely filterable through bibulous or unsized paper, indicative of the total solution of the rosin present. This dissolved state of the rosin was further corroborated by there being no deposition of solid matter on the exposure of the filtered liquid. The size was, moreover, not decomposed, or its dissolved condition disturbed by extensive dilution with water, as might be anticipated from its having the constitution of a soluble soap. The size so prepared was extremely susceptible of decomposition on the addition of very dilute acids or solution of acid salts, as alum, and was completely clarified when treated with very dilute alkalies. These reactions showed that the size was in the best condition for its intended use, viz. for the precipitation of the rosin, on being brought in contact with alum in a very diluted form in the engine, so as to size the pulp. Though this decomposition of the size and precipitation of rosin, on the addition of alum, is ultimately necessary for the sizing process, still, it is an important point for consideration, which should precede the other, the alum or the size? or, under what circumstances the deposition of the solid matter would prove most advantageous and effective for the sizing of the paper? This, in reference to rosin-size, where no antiseptic influence is necessary, seems to have been an open, and, apparently, an indifferent question. Dr. Ure and others describe them as being mixed together before adding either to the sizing engine. Many manufacturers, as the Herbertshire Mill Company, placed the rosin-size in the engine first, and subsequently the alum; a system quite right in principle if the size were of animal origin, and required the presence of excess of an antiseptic agent, as alum, to prevent any after decomposition. Veiwing, as the author did, the alum, when acting in rosin-sizing, as similar to a mordant in dyeing with adjective colouring matters, it was presumed that the alum should precede the size. *This opinion* was confirmed by the results of two experiments, viz.:—

Experiment I.—When a portion of bibulous or unsized paper, was folded as a filter, and soaked with the alum as used in sizing, and then the filter, in its wetted condition, filled with size solution as used, the passage of the latter through the filter was not permitted for many hours, and after that, it passed but in a very partial manner.

Experiment II.—This was conducted as before, only the bibulous paper was soaked with size as used, and the filter so treated filled with solution of alum as used in sizing. The paper, in this experiment, permitted the passage of the alum solution with considerable freedom, the filter being emptied in less than one hour, showing an increased permeability over the previous case.

These results indicated the order in which the ingredients should be employed, that the alum should precede the size, as the paper pulp, after being saturated with the alum, decomposes the size subsequently brought in contact with it, the solid rosin being deposited within the pores of the fibres, or in close proximity to them. The introduction of the alum first into the engine, was made trial of at the Herbertshire Paper Mill in the beginning of May, last year, half an-hour being allowed before adding the size, and the result was in the highest degree satisfactory. Specimens of paper, sized by both processes, and shown the Society, exhibited a manifest superiority in the manufactured article. As this modification of the customary process has had twelve months' trial, at the rate of 4000 lbs. weight of paper daily, its regularity has been sufficiently tested.

In conclusion, as the alum, so far as rosin-sizing is concerned, is only useful in decomposing the alkaline solution of rosin, it appeared to Mr. Macadam that a more economical agent might be employed, which, while it combined equal safety to the pulp, would prevent the passage of salts of potash into the paper. From the results of a few experiments, acetate of alumina appeared to be a suitable substance, especially as the acetate of soda, which would be formed on the decomposition of the size, is an efflorescent salt under ordinary circumstances, and would have little or no tendency to cause subsequent dampness in the paper. Besides, any slight excess of acetate of alumina remaining in the pulp would, in most part, be decomposed, with precipitation of alumina, by the temperature of the heated cylinders attached to the machine, and over which the paper passes when being deprived of moisture. The red liquor of commerce offered itself as a cheap and abundant source of this compound; but, on examination, it was found incapable of serving the intended purpose, from its containing very considerable quantites of oxide of iron and tarry matter. These tinged the size, bestowing upon it a very decided and objectionable colour, while the latter, gave it besides, an unpleasant and persistent odour. The author had some hopes that the sulphate of alumina of the alum works, just before its conversion into alum, might be found of sufficient purity to warrant the preparation of an acetate from it, by the addition of acetate of lead. Several gallons *of this liquor* were therefore obtained from the Campeie

works. The liquor was a very concentrated one, having a specific gravity of 1.342, and was certified as being as free from salts of iron as was obtainable, having been evaporated to a greater degree of concentration than usual, so as to remove all the copperas possible. The thorough separation of the salt of iron is, however, a difficultly obtainable result, since the two sulphates occasionally combine and form a double salt. On analysis it was found still heavily charged with protoxide of iron, containing fully six ounces of the anhydrous protosulphate in each imperial gallon. The inevitable peroxidation of this quantity would no doubt have discoloured the paper. On concentrating the solution to a slightly greater extent than received, in the endeavour to deposit a further portion of the iron salt, the whole liquor solidified on cooling, so that this anticipated source of alumina had to be set aside.

For the purpose of making trial of acetate of alumina as a substitute for alum, in the rosin-sizing process, without regard to economy, a quantity of the acetate was prepared from pure acetate of lead and alum. The proportions required for the perfect decomposition of these two compounds were found by experiment to be, 500 of the lead salt, to 315 of alum, or, stated in more practical proportions, 5 lbs. of the former to 3 lbs. 2 oz. of the latter, 3 gallons of water being employed, and the addition of 2 oz. of crystallised carbonate of soda to the materials after their mutual decomposition. These proportions are somewhat different from those generally adopted by calico printers and others, in the preparation of acetate of alumina from this source. The result of the application of the acetate of alumina so prepared, was a paper more thoroughly sized, and consequently *heavier* sized, than by any process previously in use, and the paper answered the purposes of the printer equally well. It, however, softened slightly in moist states of the atmosphere, owing to the presence of the deliquescent acetate of potash, from the use of alum in the preparation of the acetate. The influence of the weather on the paper so sized, was less than on a quantity prepared by using "concentrated or patent alum," a substance now being experimented with at some paper manufactories, as at Dickinson's in London. This patent alum, on analysis, was found to be principally composed of sulphate of alumina with a small proportion of soda salts. From the results of the experiments with the acetate of alumina, its use in the sizing prosess would, Mr. Macadam anticipates, be found worthy of the attention of paper manufacturers, were an economical and pure source of alumina at command.

The following report was read:—

19th December, 1849.—*Botanical Report.*

At a meeting of the Botanical Section on the 11th instant, Dr. Walker Arnott made some observations on two American trees, exhibiting an anomalous mode of growth. Mr. Keddie reported that the Botanical *Section had elected* the same office-bearers as last year.

Dr. Walker Arnott made some observations on the supposed occurrence of *Achillæa tormentosa*, near Belvie in Dumbartonshire, and on the hills near Paisley, as mentioned by Hopkirk. The probability is, that the plants found at these stations were specimens of Achillæa millefolium in a young state. The plant was also reported in Mr. Babington's manual, as found in Banffshire. Dr. Arnott had carefully investigated into this statement, and ascertained that there was every reason to believe that the plant had been introduced at a former period from the south of Europe, and that the specimen seen by Mr. Babington had sprung from a root dug up in the progress of some alterations in a garden where it had originally been cultivated. Dr. Arnott added, that, about twenty years ago, this was a common plant in gardens and nurseries. Mr. Leeshing stated that this plant was a favourite of the common people in Germany, who cultivated it for medicinal purposes. Dr. George Macleod exhibited specimens of *Smilax officinalis*, *S. sarsae*, and other species of Sarsaparilla, in a state better fitted for botanical examination than is usual with the specimens imported for commercial purposes.

ERRATUM.

Page 77, 7th line from bottom, *for* "The following paper was agreed to," *read* "The following paper was read."

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PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

FORTY-NINTH SESSION.

6th November, 1850.—*The* PRESIDENT *in the Chair.*

THE Forty-ninth Session of the Philosophical Society of Glasgow was opened this evening.

Mr. Gourlie, on the part of the deputation nominated at last meeting to represent the Society at the British Association, reported that the appointment had been fulfilled.

Mr. Gourlie presented the following donations to the Library, viz.:—From Dr. Hugh F. Cleghorn, of the Hon. East India Company's Service, paper "On the Hedge Plants of India." Thanks voted. From William John Macquorn Rankine, Esq., civil engineer, "On an Equation between the Temperature and the Maximum Elasticity of Steam and other Vapours." "Account of the effect of a Storm on Sea-walls or Bulwarks on the coast near Edinburgh." "On the Hypothesis of Molecular Vortices, and its Application to the Mechanical Theory of Heat." "Experimental Inquiry into the Advantages attending the Use of Cylindrical Wheels on Railways." "On a Formula for Calculating the Expansion of Liquids by Heat." "On the Mechanical Action of Heat, especially on Gases and Vapours."—Thanks voted.

The following paper was read:—

XVII. *Biographical Account of Dr. Wollaston.* By THOMAS THOMSON, M.D.

WILLIAM HYDE WOLLASTON, one of the most eminent chemists that Britain has produced, was born on 6th of August, 1766. He belonged to a Staffordshire family, distinguished for several centuries for their successful cultivation of science. The well known work, entitled "The religion of nature delineated," was the production of his great-grandfather. His father, the Rev. Francis Wollaston, of Chapelhurst in Kent, was an astronomer. He made an extensive catalogue of the northern circumpolar stars. He was the author of ten papers, chiefly astronomical, which appeared in the Philosophical Transactions between 1769 and 1796.

Dr. Wollaston was one of seventeen children, all of whom lived to the age of manhood. His mother was Althea Hyde, of Charterhouse Square, London. He was born at East Dereham, a village about sixteen miles from Norwich. After the usual preparatory education he went to Cambridge, and entered at Caius College, where he made great progress. He did not graduate in arts, but took the degree of M.B. in 1787, when he was twenty-one years of age. In 1793 he took the degree of M.D., being of the age of twenty-seven. At Cambridge he resided till 1789, devoting himself chiefly to astronomy—a taste which he probably imbibed from his father. He was chosen a fellow of Caius College soon after taking his degree, and this fellowship he retained till his death.

After acquiring the requisite preliminary knowledge, he settled at Bury St. Edmunds, in Suffolk, as a physician. But his success as a practitioner was so bad, that he soon after left that place and went to London. Soon after, a vacancy occurred in St. George's Hospital, and Dr. Wollaston and Dr. Pemberton started as candidates for the office of physician. Dr. Wollaston was particularly ill qualified for canvassing, and almost, as a matter of course, was unsuccessful. This want of success he took so much to heart, that he renounced the practice of medicine, and declared to his friends that he would never write another prescription. Indeed, he never liked the profession; nor was it well suited to his peculiar turn of mind. He turned his attention to science, and having discovered a method of welding the grains of platinum into metallic bars, became a manufacturer of this metal on an extensive scale, and gradually acquired a handsome fortune.

He has been accused of avarice, but apparently without reason. His brother wrote him to request him to apply to the ministry of the time being, for some situation (probably in the church) on which he had set his heart. Dr. Wollaston replied that he had never applied for any thing for himself, and could not think, therefore, of applying for another. But, continued Dr. Wollaston, if the enclosed bill be of any service to you, you are perfectly welcome to it. This enclosure was a bank bill for £10,000.

He was elected a member of the Royal Society in the year 1793, and soon became one of the most active and distinguished members of that scientific body. He and Davy became the two secretaries; and Dr. Wollaston contributed no fewer than thirty-nine papers, which were published in succession in the Philosophical Transactions; fourteen of these were upon chemical subjects, ten on subjects connected with optics, the remaining fifteen on miscellaneous subjects.

Dr. Wollaston enjoyed uninterrupted health for many years; but about two years before his death, which happened on the 22d of December, 1828, at the age of sixty-two, he was afflicted with a disease of the brain. After death, it appeared that the portion of the brain from which the optic nerve arises was occupied by a large tumor. In spite of this extensive cerebral disease, Dr. Wollaston's faculties remained unclouded to the last. His powers of vision were exceedingly perfect. I

have seen him write on paper and upon glass in so small a hand, that it seemed to be merely a single line drawn across; but when examined by a microscope it assumed the form of regular letters, distinctly visible and easily read. This power he retained to the last. When he was nearly in his last agonies, one of his friends having observed, loud enough for him to hear, that he was not at that time conscious of what was passing around him, he made a sign for a pencil and paper, which were given him. He wrote down some figures, and after casting up the sum returned them. The account was right.

In the June before his death he was proposed as a member of the Astronomical Society of London; but according to the rules of that Society he could not have been elected before the last meeting for the year. When the Society met in November, 1828, the alarming situation of his health, and the great probability of his dissolution previous to the December meeting, induced the council at once to recommend to the assembled members a departure from the established rule, and that the election should take place at that sitting. This was done, and received the unanimous sanction of the meeting, which insisted on dispensing with even the formality of a ballot. Dr. Wollaston then, within a few days of his death, acknowledged this feeling and courteous act by presenting the Society with a valuable telescope which he greatly prized. It originally belonged to his father, and had been subsequently improved by the application to it of an invention of his own, the triple achromatic object glass—a device on which astronomers set great value.

At the death of Sir Joseph Banks, Dr. Wollaston was chosen as interim president from the time of that death, to the 30th November of the same year, which was the usual time for the election of the president. Not a few of the members were anxious that he should have succeeded Sir Joseph Banks as president, but he peremptorily refused to allow himself to be put on the list of candidates. The consequence was, that Sir Humphrey Davy was chosen to fill that important office without opposition.

Towards the latter part of 1828, Dr. Wollaston became dangerously ill of the disease of the brain of which he died. Finding himself unable to write out an account of such of his discoveries and inventions as he was reluctant should perish with him, he spent his numbered hours in dictating to an amanuensis an account of some of the most important of them.

The chief of these is indisputably his method of rendering platinum malleable, which he had practised for many years upon so large a scale, that he is said to have cleared thirty thousand pounds by that process alone. He had ascertained the fact that platinum, like iron, is capable of being welded. Hence he inferred that it might be converted into a metallic rod or plate, susceptible, by skilful hammering, of being converted to vessels of any shape or size required. As it is capable of resisting the greatest heat of our furnaces, and is not acted upon by the reagents

employed in chemical experiments and analysis, its immense importance in chemical researches became at once obvious.

But native platinum is a compound or mixture of eight different metals. It was necessary to get rid of these foreign bodies before converting platinum into bars. He dissolved crude platinum in nitro-muriatic acid, filtered, and precipitated the platinum by sal-ammoniac. The yellow precipitate was carefully washed, and heated very cautiously in a black lead crucible to drive off the sal-ammoniac. The grey residue is platinum. It is rubbed to powder by the hand, and then triturated by a wooden pestle in a wooden mortar, care being taken to do nothing that would polish the edges of the platinum powder, because that would prevent the welding process from taking place. The powder is now put into a brass mould, filled with water, taking care that no vacuities are left. The top of the powder is covered first with paper and then with cloth, and it is then compressed with the force of the hand by a wooden plug. After this a circular plate of copper is placed on the top, and it is exposed to a very violent pressure in a horizontal press. It is then put into a charcoal fire and heated to redness, to drive off the water.

The ingot of platinum thus formed is placed upon an earthen stand, about $2\frac{1}{2}$ inches above the grate of a wind furnace. The ingot is placed on its end, and is exposed for twenty minutes to the highest temperature that can be raised in the furnace. It is now placed on an anvil, and struck while hot on the top with a heavy hammer, so as at one heating effectually to close the metal. It must never be struck on the sides, which would cause it to crack. By this hammering it is brought into the state of a perfect ingot fit for all purposes.

During Dr. Wollaston's experiments on crude platinum, he discovered a new metal, to which he gave the name of *palladium*, or new silver. In the year 1803 he drew up a statement of the most remarkable and characteristic properties of palladium. This statement, together with some specimens of the metal, was exhibited in the windows of some shops in London, without the least hint of who the discoverer was, or from what source the metal was obtained. This very uncommon mode of exhibiting a chemical discovery, naturally led Mr. Chenevix to suspect that the pretended new metal was nothing else than an artificial compound of some metals previously known. He purchased, accordingly, all the specimens of palladium exhibited in London for sale; and after an elaborate and laborious course of experiments, drew up a paper on the subject, in which he showed that it was an amalgam of platinum, or a compound of mercury and platinum. This paper was read at a meeting of the Royal Society, and unless I am mistaken, Dr. Wollaston himself was the person that read it to the Society.

Chenevix's paper was not only read to the Royal Society, but published in their transactions for 1803, without any information afforded that the metal called palladium had been discovered and examined by Dr. Wollaston. On taxing Dr. Wollaston with cruel and unhandsome conduct

for not intrusting the secret to Mr. Chenevix, he assured me that he had done all in his power to convince Chenevix that he was mistaken—that he had written him, assuring him that he himself had repeated Mr. Chenevix's experiments and found them inaccurate, and that he himself was satisfied, from careful examination, that palladium was a distinct metal. I have no doubt that Wollaston's statement is correct, but think that he ought not to have allowed Mr. Chenevix to publish his paper, without betraying the secret of the discovery of palladium, and the reasons which induced him to believe that palladium was a peculiar metal. Chenevix had been occupied at the rate of fourteen hours a day for nearly a quarter of a year. It is not surprising that he was not likely to yield to a set of experiments differing from his own. But the effect of Dr. Wollaston's conduct was to destroy the chemical reputation of Chenevix, and put an end to the chemical career of one of the most active and laborious chemists of his time.

In the Philosophical Transactions for 1804, Dr. Wollaston published an account of the properties of palladium, and pointed out the mistake into which Chenevix had fallen. In the same paper he described the properties of another new metal which he had found in crude platinum, and to which he gave the name of *rhodium*.

It will be worth while to take a short review of Dr. Wollaston's chemical papers, published in the Philosophical Transactions, that we may see the discoveries for which chemistry is indebted to him.

1. The earliest of these discoveries, though the last given to the chemical world, was the method of rendering platinum malleable and ductile. It furnished practical chemists with a most important utensil, to which chemistry is indebted for the great degree of perfection to which chemical analysis of minerals has reached. Every body now can analyse a mineral with tolerable accuracy; but before Dr. Wollaston supplied a platinum crucible, the analysis of the simplest mineral was a work attended with great labour, and a great waste of time.

All the great improvements in chemistry were preceded by the discovery of certain utensils, which, when applied to chemistry, developed a new series of important facts. The pneumatic apparatus contrived by Cavendish and Priestley, led to the discovery and examination of numerous elastic fluids which had hitherto escaped the attention of chemists. Dr. Wollaston's platinum crucibles speedily brought the art of analysing minerals to a state of perfection. The discovery of the galvanic battery, and the decomposing power of electricity, led Davy to the discovery of the constitution of the fixed alkalies, alkaline earths and earths proper, which had previously been considered as simple substances. The simplification and perfection by Liebig of the apparatus contrived by Gay Lussac and Thenard for the analysis of vegetable bodies, led immediately to the examination of an immense number of substances of vegetable origin, and the discovery of numerous interesting and important bodies which had hitherto escaped the attention of chemists.

2. It is well known to every individual who takes any interest in chemical investigations, that what is called Dalton's atomic theory was made known to the public about the year 1804. According to that theory every simple substance is an atom having a determinate weight. Bodies combine either atom to atom, or an atom of one with a certain number of atoms of another. At that time chemists were in possession of hardly any accurate analysis of salts or of chemical compounds in general. Mr. Dalton founded his theory on the analysis of two gases, namely, protoxide and deutoxide of azote; the first consisting of a certain quantity of azote united with a determined weight of oxygen, the second of the same quantity of azote united to twice as much oxygen. The first of these he considered as a compound of one atom of azote with one atom of oxygen, and the second of one atom of azote united with two atoms of oxygen.

In the year 1808 I supplied Mr. Dalton with two instances of similar combination, namely:—

1. Oxalate of potash.
   Binoxalate of potash.
2. Oxalate of strontian.
   Binoxalate of strontian.

After the perusal of my paper on oxalic acid, Dr. Wollaston read a paper to the Royal Society *on super-acid* and *sub-acid salts*, which was published in the Philosophical Transactions for 1808. In this paper he gives six examples of similar combinations, namely:—

1. Carbonate of potash.
   Bicarbonate of potash.
2. Carbonate of soda.
   Bicarbonate of soda.
3. Sulphate of potash.
   Bisulphate of potash.
4. Oxalate of potash.
   Binoxalate of potash.
   Quadroxalate of potash.

3. About the beginning of the present century, Mr. Hatchett discovered a new metal in a mineral from America, a specimen of which was in the British Museum. To this new metal he gave the name of *columbium*. Soon after Mr. Hatchett's discovery, a metallic substance was detected in Sweden by Mr Ekeberg, differing from every other with which he was acquainted. This new metal he distinguished by the name of *tantalum*. The discovery of Hatchett was made known to the public in the Philosophical Transactions for 1802, and that of Ekeberg in the memoirs of the Swedish Academy of Sciences for 1802.

In the year 1809 Dr. Wollaston procured specimens of the Swedish mineral containing *tantalum*, and of the mineral in the British Museum containing *columbium*, extracted a little of the oxide of tantalum from the one, and of the oxide of columbium from the other, and by a very ingenious comparison of the two, demonstrated that both oxides are identical, and that columbium and tantalum constitute one and the same metal. These results were published by Wollaston in 1809 in the Philosophical Transactions, and exhibit a very satisfactory display of his

mode of experimenting on a minute scale, and of the sagacity which enabled him to draw the proper conclusions from very simple premises.

4. Dr. Wollaston's discovery regarding *titanium*, ought not to be passed over in silence. Titanium is the name given by Klaproth to a new metal discovered by Mr. Gregor in the valley of Menachan in Cornwall, and called on that account *menachine*. Mr. Gregor published an account of his discovery in 1791. In the year 1795 Klaproth discovered a new metal in a mineral at that time distinguished by the name of red schorl, to which he gave the name of *titanium*. And in 1797 he made a comparative set of experiments on the *menachine* of Gregor and his own *titanium*, by which he established the identity of these two metals with each other. All attempts to reduce the oxide of titanium to the metallic state failed, if we except the small quantity of metallic titanium extracted by Vauquelin and Hecht in 1796, and the subsequent method of reducing the oxide to the metallic state contrived by Liebig in 1831, and deduced by him from Henry Rose's experiments on ammonio-chloride of titanium.

Red cubes having the metallic lustre, are occasionally discovered in the slag of the hearths of the great iron smelting houses, so abundant in this neighbourhood and in Wales. These cubes were examined by Dr. Wollaston in 1822, and shown to possess the characters of titanium in the metallic state. He found the cubes to consist of metallic titanium of the sp. gravity 5·3, and to be hard enough to scratch rock crystal.

Such was the state of our knowledge of these cubes of metallic titanium, as was supposed, when Wöhler published an elaborate set of experiments on them in the year 1850. He showed that they always contained graphite mechanically mixed. By a very ingenious but complicated set of experiments, Wöhler showed that the metallic cubes of supposed titanium were, in fact, composed of titanium, azote, and carbon, in the proportions—

Titanium,	78.00
Azote,	18·11
Carbon,	3·89

The carbon was combined with azote, constituting cyanogen, while the remainder of the azote was united with the titanium, constituting an azotide. The crystals, according to these experiments, are composed of—

Cyanide of titanium,	16·21
Azotide of titanium,	83·79
	100·00

or,

1 Atom cyanide of titanium.
3 Atoms azotide of titanium.

In the year 1823 when Dr. Wollaston's paper was published, the science of chemistry was not far enough advanced to enable him to make a complete and satisfactory analysis of this very remarkable compound.

5. The next paper of Dr. Wollaston which I shall notice, was inserted in the Philosophical Transactions for 1814, and was entitled a *Synoptical Table of Chemical Equivalents.* It had been observed by Richter, that when solutions of two neutral salts which decompose each other are mixed, the new salts formed are always equally neutral. Thus, if 9 parts of sulphate of soda be mixed with 16·25 parts of nitrate of barytes, the two neutral salts will be converted into 10·5 parts of nitrate of soda and 14·5 of sulphate of barytes, the sulphate of barytes will precipitate, and the nitrate of soda will remain in solution. Richter found this law to hold in all the cases tried, and thence inferred that the ratios of saturation of acids and bases, were always the same. Thus, 4 by weight of soda will just saturate 5 of sulphuric acid, 6·75 of nitric acid, 7·25 of arsenic acid, 4·5 phosphoric acid. Dr. Wollaston explained this remarkable property by means of the atomic theory of Dalton. Acids and bases unite atom to atom, or one atom of the one with 1 or 2 or more atoms of the other. He showed, by a most laborious investigation, that the same law holds in all chemical combinations. Metals combined with one two or more atoms of oxygen to form oxides, with two or more atoms of sulphur to form sulphurets. Every body has a peculiar atomic weight. This he determined in a very considerable number; drew up a table of atomic weights referred to oxygen as unity, and transferring them to a sliding scale, enabled the practical chemist to see at once the weight of any body necessary to saturate an atom of any other. These scales were exposed to sale, and at one time were very common in laboratories. But the vast number of names upon the scale, made it difficult to discover the name wanted, and on that account they have gradually gone out of use.

6. In the year 1813 a paper by Dr. Wollaston was published in the Philosophical Transactions, giving an account of a very ingenious mode of showing the cold induced in water by evaporation. To the apparatus used he gave the name of *cryophorus.* It consisted of a glass tube about $\frac{1}{8}$ of an inch in diameter, terminating at each extremity in a glass ball about one inch in diameter. This tube was bent at a right angle about half an inch from each ball. One of these balls should contain a little water. This water is boiled till all the air is driven out of the balls and tube. The tube is now hermetically sealed and allowed to cool. The water is then collected in one ball, while the other at the distance of the tube, is plunged into a freezing mixture. By this contrivance the vapour as it rises from the water is condensed, and thus the evaporation from the water is continued unabated. The cold generated by this evaporation is so great, that in a few minutes the water in the remote bulb is converted into ice.

7. There is still a paper of Dr. Wollaston's to be noticed. I mean his examination of urinary calculi. It was published in the Philosophical Transactions for 1797, and was indeed the first of his publications. One species of urinary calculi had been examined by Scheele, who showed that it was composed of a peculiar acid substance which exists in urine,

on which account it got the name of *uric acid calculus*. Dr. Wollaston analysed 4 new species of calculus, and determined the composition. These were:—

1. *Fusible calculus*. This calculus before the blow-pipe fused into an opaque white glass. It is a mixture of phosphate of lime, and ammonia-phosphate of magnesia. 2. *Mulberry calculus*.—So called by surgeons because it has a brown uneven surface, having some resemblance to a mulberry. It consists essentially of *oxalate of lime*. 3. *Bone earth calculus*. It has a brown colour and a smooth surface. It consists essentially of phosphate of lime, and differs from bone earth by containing no carbonate of lime. 4. In 1810 Dr. Wollaston discovered a new calculus, to which he gave the name of cystic oxide calculus. 5. Gouty concretions, composed of urate of soda.

8. Such is a meagre catalogue of Dr. Wollaston's chemical papers published in the Philosophical Transactions; there is still another notice by him which deserves to be stated.

Dr. Marcet, at the time of his death, was occupied with a set of experiments to determine the quantity of salt in the Mediterranean sea, and with endeavouring to account for the constant influx of the Atlantic ocean by the straits of Gibraltar, without any sensible increase of the specific gravity. He had applied to Captain William Henry Smyth, who was engaged in surveying part of that sea, to supply him with water at great depths from that sea. Dr. Marcet dying before he received the water expected, Captain Smyth gave to Dr. Wollaston three bottles from the bottom of the sea, and at different distances from the Straits of Gibraltar. The first two specimens were taken from 680 miles, and 450 miles from the Straits, at the depths of 450 and 400 fathoms, contained water of the usual specific gravity, namely, 1·0294 and 1.0295. But the third, taken 50 miles from the strait, and at a depth of 670 fathoms, had a specific gravity of 1·1288. The first two contained 4 per cent. of salt, the last 17·3 per cent. It is clear from this, that an under current outward, if of equal breadth and depth with the current inward at the surface, would carry as much salt below as is brought in above, although it moved with $\frac{1}{4}$ part of velocity, and would thus prevent any increase of salt in the Mediterranean beyond what exists in the Atlantic.

The remaining papers by Wollaston in the Philosophical Transactions, amount to 25. They are on various subjects, all ingenious, and each containing a new fact.

Dr. Wollaston's Papers in Philosophical Transactions.

1. On Gouty and Urinary Concretions, vol. 87, p. 386, 1797.
2. On Double Images by Atmospherical Refraction, vol. 87, p. 239, 1800.
3. Experiments on the Chemical Production and Agency of Electricity, vol. 87, p. 427, 1801.

4. A Method of Examining Refractive and Dispersive Power by Prismatic Reflection, p. 365, 1802.
5. On Oblique Refraction of Iceland Crystal, p. 381, 1802.
6. Quantity of Horizontal Refraction, &c., p. 1, 1803.
7. On a New Metal (palladium) in Platina, p. 419, 1804.
8. On the Discovery of Palladium, p. 316, 1805.
9. On the Force of Percussion, p. 13, 1806.
10. On Fairy Rings, p. 133, 1807.
11. On Superacid and Subacid Salts, p. 96, 1808.
12. On Platina and Native Palladium from Brazil, p. 189, 1809.
13. Identity of Columbium and Tantalum, p. 246, 1809.
14. Description of a Reflective Goniometer, p. 253, 1809.
15. On the Duration of Muscular Action, p. 2, 1810.
16. On Cystic Oxide, p. 223, 1810.
17. On the Non-existence of Sugar in the Blood of persons labouring under Diabetes Mellitus, p. 96, 1811.
18. Crystals of Carbonate of Lime, Bitter Spar and Iron Spar, p. 159, 1812.
19. On a Periscopic Camera Obscura, &c., p. 370, 1812.
20. On the Elementary Particles of certain Crystals, p. 51, 1813.
21. Method of Freezing at a Distance, p. 71, 1813.
22. Drawing very fine Wires, p. 114, 1813.
23. Single Lens Micrometer, p. 119, 1813.
24. Synoptic Scale of Chemical Equivalents, p. 1, 1814.
25. On Cutting Diamonds, p. 265, 1816.
26. On Native Iron in Brazil, p. 281, 1816.
27. Cutting Rock Crystal for Micrometers, p. 126, 1820.
28. Sounds Inaudible to certain Ears, p. 306, 1820.
29. Concentric Adjustment of Triple Object Glass, p. 32, 1822.
30. On the Finite Extent of the Atmosphere, p. 89, 1822.
31. On Metallic Titanium, p. 17, 1823.
32. Magnetism of Metallic Titanium, p. 400, 1823.
33. On the Semidecussation of the Optic Nerves, p. 222, 1824.
34. Apparent direction of the Eye in a Portrait, p. 247, 1824.
35. Method of making Platinum Malleable, p. 1, 1829.
36. Microscopes Double, p. 9, 1829.
37. Comparing the Light of the Sun and Fixed Stars, p. 29, 1829.
38. Water in the Mediterranean, p. 29, 1829.
39. Differential Barometer, p. 133, 1829.

20th November, 1850.—*The* President *in the Chair.*

Mr. Liddell gave in the following Abstract of the Treasurer's Account, which, having been printed in the circular calling the meeting, was held as read, and unanimously approved of:—

1849.	Dr.						
Nov. 11.—	To Cash in Union and Provident Banks,.....	£91	15	1			
	— Interest on do.,.....	1	8	0			
1850.					£93	3	1
Nov. 1.	— 48 Entries of New Members,...	50	8	0			
	— 13 Annual Payments from Original Members, at 5s.,.....	3	5	0			
	— 246 Annual Payments at 15s.,..	184	10	0			
	— 4 do. arrears of one year, each 15s.,.....	3	0	0			
					241	3	0
					£334	6	1
1849.	Cr.						
Nov. 11.—	By balance due Treasurer,.....				£1	17	10
1850.							
Nov. 1.	— New Books and Binding,.....				117	19	8
	— Printing Transactions, Circulars, &c.,.....				40	16	0
	— New Table and Repairs on Hall,.....				9	11	0
	— Rent of Hall,.....£15	0	0				
	— Coffee for Evening Meeting,.....	4	7	0			
	— Fire Insurance,.....	2	16	0			
	— Society's Officer, Clerk, Addressing Circulars, and Poundage collecting dues,.....	12	2	0			
	— Postages, Delivering Letters, Stationery, &c.,.....	12	8	9			
	— Gas for Hall, &c.,.....	1	0	0			
					47	13	9
	— Sub-Librarian's Salary, 43 weeks, at 6s.,.....	12	18	0			
	— Dress for ditto,.....	3	2	6			
					16	0	6
	— Subscription to Ray Society, 1 yr..	1	1	0			
	— Subscription to Cavendish Society, 1 year,.....	1	1	0			
	— Subscription to Palæontographical Society, 2 years,.....	2	2	0			
	— Donation to Clydesdale Natural History Society,.....	6	0	0			
					10	4	0
	By Cash in Union Bank,.....	90	0	0			
	— Do. in Savings' Bank,.....	0	3	4			
					90	3	4
					£334	6	1

THE MODEL EXHIBITION FUND.

1849.

May 15.—To balance as per deposit Receipt from the Corporation of the City of Glasgow,.....£505 16 1

1850.

May 15.—To one year's Interest on do.,..... 24 0 8

Amount in the hands of the Corporation,.....£529 16 9

GLASGOW, *14th November*, 1850.—We have this day examined the Treasurer's Accounts, and compared the same with the Vouchers, and find that there is in the Union Bank of Scotland the sum of Ninety Pounds, and in the Savings' Bank Three Shillings and Fourpence, making a balance of £90 3s. 4d. in favour of the Society. The Treasurer has also exhibited to us a Voucher for £529 16s. 9d. which he holds for money lent to the City of Glasgow, from the proceeds of the Exhibition in the year 1846, with interest to 15th May last.

RICHARD S. CUNLIFF.
WILLIAM COCKEY.

Report by the Treasurer, 6th November, 1850.—The expenditure has exceeded the income of the Society this year to a small amount, notwithstanding the great increase of revenue from new members; their number admitted during last Session being nearly double that of any former year. The increase of expenditure this year has been entirely in the Library department. The balance in Bank of £90 3s. 4d. is under liabilities to booksellers and others to nearly £50. The number of Members on the Roll at commencement of Session was 231; new Members admitted, 48; making in all 279. Of these there have resigned by letter, 3; dead, 4; struck off list for arrear of dues, 8; removed from the ordinary to the non-resident list, 4; and removed to list of Honorary Members, 1; making in all 20—which leaves 259 in all on the list of Ordinary Members at this date. The Treasurer has to report that but few of the Members who remove their residence from Glasgow give intimation of their having done so, or express any wish that their name may be retained on the non-resident list. As he believes that in many instances this arises from oversight, he will now quote what was stated in his Report of last year:—"Had the Members who have removed their residence from Glasgow expressed a desire conform to Law XI., that their names should be retained on the list of Non-Resident Members, their privilege would have thereby been reserved of '*resuming their position as Resident Members whenever they return to Glasgow, upon payment of the current year's subscription.*' By neglect of giving this intimation, the name is retained on the general roll, and dues exacted till the expiry of two years, at which period, if not paid, the name is then expunged from the list, and cannot be restored without a new election, and one guinea of entry-money paid." All dues exigible must be paid up to the period of giving such intimation, before the name can be placed on the non-resident list.

In compliance with Law VI., the Treasurer now reports that the only property possessed by the Society is the Cash in Bank, as per Account current now presented; the Books in the Library, and Book Presses, as per Librarian's Catalogue; Portrait of President, in Gilt Frame; Marble Bust of President; President's Chair of Oak from roof of Cathedral; Hall Table, in two pieces; Writing Desk, one do.; Four Gas Lustres; Black Board; Stove in upper Library-room; Eight Benches with Top Rails; Steps for Book-cases; Ballot-Box; Secretaries' Charter Chest; Treasurer's do.; and a Portable Table for Evening Meetings.

The Librarian reported that the Society was now in the receipt of 27 English, 10 French, and 4 German periodicals. One German journal has been completed, viz., Erdmann's Journal für Praktische Chemie, by the purchase of 36 vols. at a cost of £11. All the volumes in the Library have been inspected, and, with one exception, viz., Whewell's Philosophy *of the Inductive* Sciences, volume 1, which is missing, all have been

found in good order. The advantage of having a person stationed in the hall to give out and receive books during the day, appears to have given general satisfaction, as is evinced by the increase in the number of books borrowed by members. The total number of volumes in the Library on the 1st of November was 1663.

The Society then proceeded to the Forty-ninth annual election of Office-bearers, having previously appointed Mr. James George Morison and Mr. Charles Watson as scrutineers of votes.

The scrutineers reported the following gentlemen to have been elected Office-bearers of the Society for the year 1850–51, viz.:—

President.

THOMAS THOMSON, M.D.

VICE-PRESIDENT,..WALTER CRUM. | LIBRARIAN,...R. D. THOMSON, M.D.
TREASURER,.....ANDREW LIDDELL. |

Joint Secretaries.

ALEXANDER HASTIE, M.P. | WILLIAM KEDDIE.

Councillors.

JAMES BRYCE.	A. MITCHELL, M.D.	PROF. WM. THOMSON.
WILLIAM FERGUSON.	WILLIAM MURRAY.	JOHN WILSON.
WM. GOURLIE.	J. STENHOUSE, LL.D.	WALKER ARNOTT, LL.D.
ALEXANDER HARVEY.	ALLEN THOMSON, M.D.	A. K. YOUNG, M.D.

The following paper was read:—

XVIII.—*Notice of a Marine Deposit containing Shells, lately discovered in Sauchiehall-Street.* By WILLIAM FERGUSON, ESQ.

THE fact which I have this evening to bring before the Society, is another proof added to the many already recorded of the existence of the sea, at former levels, higher than those it now occupies. Mr. Smith of Jordanhill has already noted many of the Clydesdale series of these proofs, in connection with their organic remains; and Mr. Robert Chambers, in his interesting book, entitled "Ancient Sea Margins," has described topographically, what he considers a series of ancient sea beaches in this neighbourhood, and connected them with others existing at similar levels, not only throughout this country, but also on the continent and abroad. The most marked of these are the Low Green and King's Park, the level of London-Street, the Trongate, and Argyle-Street, the level on which Elmbank Crescent is built, and that occupied by the College. He also mentions another which he traces around Garnethill, and it is more especially to what may probably be a part of this one, I now wish to call your attention. I premise, however, that I do not conceive Mr. Chambers concluded the ancient beach he refers to, occupied the higher level of Sauchiehall-Street; I rather understand him to confine it to the lower portion of Sauchiehall Road, where Albany Place and the Rows westward are built.

The present deposit was cut into in the line of Sauchiehall-Street, 25

paces westward from the entrance to the Wellington Arcade, and opposite No. 134. Between this and the western part of the road there was a considerable eminence opposite Clarence Place, which has now been very much lowered; previous to this the ground must have risen from towards George Square by the head of Buchanan-Street, and so westward, reaching its highest level at Clarence Place, with a hollow along the line of Sauchiehall-Street, and a ridge existing to the south, the water-shed of which lies between Bath-Street and West Regent-Street. This ridge terminates westward at Blythswood Square, and I am informed that originally the site of the square was very much higher than it is at present, and that the descent from Bath-Street to Sauchiehall-Street, was very much more abrupt than now, the material cut from the top of the hill in levelling the square, having been used to fill up eastwards. It would be very interesting to trace the alterations which have been made on the features of our city's locality by these levelling operations, but this does not come within my province at present, farther than what is necessary to form an idea of the physical geography of the spot at the remote period to which these shelly deposits carry us back.

Keeping in view, then, the extent to which these levelling operations have been carried on, in and around the very spot with which we are now dealing, it is not a little surprising, that so recently as within two months, in opening an excavation for a sewer, a section such as that now represented should have been exposed.

Section of Excavation in Sauchiehall-Street.



Causeway and sand,.....	1	2
Clay and mould disturbed,	2	0
Clay and mould undisturbed,.....	1	0
Black moss or peat,.....	1	0
Coarse and fine sand beds or layers, 2 to 4 inches each, containing shells, exposed to the depth of 4 feet, but not passed through,.....	4	0
Feet, ...	9	2

There is a sewer in the street, joined at this point by a side drain, from a well in the back court of the houses immediately west of the Arcade. This side drain had got choked, and required to be opened. Its exact position was not accurately known, and was missed. The excavation was made past the original sewer, and broke up new ground.

After removing the causeway and sand to the depth of fourteen inches, three feet of clay and mould were passed through. Of this two feet were evidently artificial, but the lower portion bore every mark of being the original soil. Below this there was one foot of black moss or peat, so pure, that some children residing close by carried it into the house for fuel, and told their parents that they were cast-

ing peats on the street. Under the peat, a depth of four feet of alternating layers, from two to four inches in thickness, of fine and coarse sand were exposed. This deposit was not perforated, and its depth is therefore unascertained. It contained shells, none of which, however, with one exception, were preserved. Several were obtained, but no value being attached to them by the workmen, they were broken and thrown aside. The one in question owed its preservation to its having attracted the attention of one of the men from the brilliant appearance of a portion of its pearly structure, apparent through the broken epidermis. The party who preserved it, Mr. James Peters, happened to be a patient of my friend Dr. Lorrain, who has favoured me with this narrative, and it struck him that this pretty shell would be an acquisition to the Doctor's Conchological Collection. It accordingly reached him, and this led to the examination and recording of this interesting discovery. The shell is *Trochus zeziphanus*, a common enough species on our shores. Mr. Peters informs me, that the workman who dug up the shells, observed five or six in one spadeful of the sand, but paying no attention to them, they were thrown out and destroyed.

An old building next the new land of houses adjoining the Arcade, is to be taken down and rebuilt in spring. This house stands on the original soil without any sunk-story. As the new houses will have such sunk-stories, we may look forward to obtaining more evidence on this subject when the foundations for the new houses come to be dug out.

Mr. Chambers' description of the Glasgow terraces, is as follows:—

"At Glasgow the river has ceased to be an estuary, though affected by the tides for three miles higher, namely, to Rutherglen. Around, and also within the city, I have found several of the ancient beaches. In Glasgow Green the same two haughs which occupy so much of the Leven Vale are distinctly seen, one of them about 11 and the other 26 feet above the ordinary level of the sea. The Trongate and adjacent districts of the city, are built upon the second of these plateaux, which also extends over a large space on the opposite side of the river; at Partick, to the west of the city, this beach is also clearly marked, being there about 26 feet high.

"The Ascog beach likewise appears in or near Glasgow, but does not pass through it so uninterruptedly. Ascending from Partick towards the Observatory, we find it at Dowanhill, and also on the east side of the Kelvin Valley. If we make a cross movement from the river bank at the Broomielaw, the following beaches will be found:—First, the street of Broomielaw, a piece of ancient haugh 10 feet above high water mark; second, another flat at Anderston-Street, at about 30 feet; third, a terrace sloping up to the skirts of Garnethill, somewhat irregular, but exhibiting some entire pieces, (for example, the site of Free St. Matthew's Church,) and attaining an extreme height of somewhat more than 80 feet. A similar cross movement in the eastern suburbs, starting at the Green, and passing up to the lodge at the House of Refuge, gives a precise repetition of these gradations. The Hill-Street Factory is thus

seated upon the 64–70 feet level. In the central part of the city we pass at once from the 26 feet alluvium (for example at George-Square) up a steep slope, to an irregular height not less than 100 feet, remarkable for a capping of diluvium, containing a number of far transported boulders. But in the line of the High-Street, the University Buildings clearly sit upon the same terrace which we find at Dowanhill and the Hill-Street Factory. On the right bank of the Molendinar Burn, opposite to the Craig Park, there is a fine piece of terrace about 150 yards in length, and perhaps 50 feet above the tiny stream. This is approximately 144 feet above the level of the sea."

There is collateral proof of the correctness of the theory of ancient beaches in the records of the discovery of several canoes, imbedded in sand, at various places on our river. For instance, in 1847, one was discovered at Springfield. It was found about 100 feet from the margin of the Clyde, and rested on a bed of gravel fifteen inches thick, covering a bed of finely laminated sand. Over it was a bed of loam nine or ten feet thick, surmounted by sand; the entire depth of the situation of the canoe below the surface was seventeen feet, being just about the level of low water in the river. Three others were afterwards found here.

Previous to this, in July, 1825, a canoe was also discovered in digging a sewer in London-Street. An account of it in the Gentleman's Magazine states that the boat lay "in a bed of blue clay, which was covered and surrounded by fine sand, like that found on the shores of a navigable river or wide frith." The author of "Glasgow Delineated" says that inside were sand and shells; and Mr. James Peters, the same man who preserved the Trochus of the Sauchiehall-Street beach, informs me that he saw the canoe dug up, and that it was covered with mussels and wilks which were adhering to the wood, and which he took off with his own hands.

There is some vague account of another of these boats having been discovered in cutting a sewer in Stockwell-Street, but nothing definite seems to be known about it.

It is stated in Chapman's Picture of Glasgow, published in 1818, p. 152, that a boat was discovered in digging the foundation for the Tontine buildings. He describes it as imbedded in sand and gravel, several feet below the surface; and one was dug out of the foundations of the original church of St. Enoch's in 1780. It was lying flat, and filled with sand and shells. In the bottom there was sticking a celt or hatchet used by the aboriginal inhabitants. The boat was seen by the late John Wilson, Esq., who secured the possession of it, and it is now the property of Charles Wilson Brown, Esq. It is in good preservation.

As to the extreme duration of the period during which these terraces were formed, Mr. Smith of Jordanhill has made the following remarks:—

"At an elevation of about forty feet, there has been observed upon many parts of our coasts a series of raised beaches and terraces, which, by their magnitude, indicate the prodigious length of time at which the

sea level must have been stationary at this height, and if we may judge of its duration from the relative size of the ancient terraces with those now forming, it must have exceeded the recent period, of which 2000 years is but a part, by an immense amount; but this is but one of the epochs in the history of this formation: between the great terrace and the sea, several subordinate ones and beaches have been observed, each of them marking long continued periods of repose, whilst a sudden deepening two or three fathoms below low water mark is probably caused by another line of terraces now covered by the sea."

The following table of the classification of the different formations of this, the pleistocene or glacial period of geology, is constructed from Mr. Smith's paper, and will help us to form an idea of the extent of time necessary for its production:—

1. Elevated marine beds. Ancient beaches.
2. Submarine forests.
3. Alluvial beds, most likely marine, but affording as yet no organic remains.
4. Upper diluvium or till, the most recent deposit of the till.
5. Marine beds in the till affording shells, at Airdrie.
6. Lower diluvium, till, or boulder clay.
7. Stratified alluvium, consisting of sand, gravel, and clay, without organic remains, resting in this district immediately upon the upper members of the carboniferous system.

I have divided the diluvium or till into two members, as certain recent observations, lately laid by Mr. Smith before the Geological Society, have shown it to have been deposited at two periods, with quiet water intervening, and this also adds indefinitely to the already almost boundless extent of time required for the development of these beds.

Without entering into any of Mr. Chambers's conclusions as to uniformity in the oscillations of level of the sea and land, or the vexed question as to whether it is the land that has risen or the sea that has fallen, we may conclude with him, that there was a time when "the Frith of Clyde was a sea several miles wide at Glasgow, covering the site of the lower districts of the city, and receiving the waters of the river not lower than Bothwell Bridge." And we may imagine that at the time when these beds of sands were being laid down, where I have described them in the hollow of Sauchiehall-Street, the waters of this noble estuary eddied around the various eminences which yet mark the physical geography of Glasgow. Garnethill would stand out conspicuously, separated by a narrow and not deep channel from Blythswood-hill. A broader and deeper current would flow betwixt it and the hill where Port-Dundas now stands, finding its way into the main channel farther westward, while to the south the wide expanse of water would sweep onward, with perhaps an islet here and there, towards the Cathkin and other southern hills, presenting more the appearance of a landlocked bay or inland sea, than an estuary. But on the remote antiquity of this era who shall speculate?

And if this, the most recent of all geologic periods is so utterly, in its limits and duration, beyond mortal calculation, what of the vast series of eras which preceded it? The indefiniteness of time which geology requires, is only equalled by the indefiniteness of space which astronomy demands, and the twain only surpassed by the infinity of Him who actively fills both with the evidences of his presence and his perfections.

December 4th, 1850.—Mr. Crum, *the* Vice-President, *in the Chair.*

The following were admitted members of the Society, viz.:—Dr. John Strang, city chamberlain, Dr. William B. Lorrain, Messrs. Robert Gray, Hugh Heugh, Hermann L. Seligmann, Roger Hennedy, James P. Fraser, John Muir Wood, James Taylor, John Macharg, James Reid, Thomas Allan, James Stein, John Inglis.

The Vice-President, in alluding to the subject of the communication of papers to the Society, stated, that although the Society had been established for the reading of original contributions on scientific subjects, it was not the less desirable that members should bring under the notice of the Society mechanical improvements and new applications of scientific principles which might not strictly come under this description.

Mr. Liddell added some remarks to the same effect, and recommended the suggestion to the attention of the members.

Mr. Bryce gave notice of a motion for a grant of money to the Clydesdale Naturalists' Association, to aid in the researches which they are at present prosecuting, and some of the fruits of which would be submitted to the Society in the paper now to be read.

Mr. Bryce then read a paper "On the Lesmahagow and Douglas Coal Field," which was illustrated by a map and section, and specimens of the fossils. This paper will form part of the General Report.

Mr. William Ferguson afterwards directed the attention of the Society to some of the more characteristic fossils collected during the survey of this portion of the coal measures.

Letters, acknowledging the presentation of No. 2, Vol. III. of the Society's printed proceedings, were received from the Secretary of the Royal Society of London, the Secretary of the Liverpool Literary and Philosophical Society, and the Secretaries of the Literary and Philosophical Society of Manchester.

December 18th, 1850.—*The* Vice-President *in the Chair.*

The following were admitted members, viz.:—Messrs. Robert M'Ghie, C.E., John Lawson, George Lyon, John Anderson, Charles M'Lean, John Finlay, Robert Taylor.

Mr. Bryce brought forward the motion of which he had given notice,

for a grant of money to the Clydesdale Naturalists' Association, to aid them in their investigations into the Lanarkshire coal field. He moved that the grant should be £15.

The motion was seconded by Mr. William Ferguson, and supported by Mr. Gourlie.

The first vote was taken, and the motion unanimously agreed to.

Mr. William Murray took occasion to state that a collection was now forming of the various minerals of the district, from the upper part of the coal field to the lowest strata, the specimens to be in cubes of six inches each, for the Exhibition in London. The collection was being made under his superintendence, and he had stipulated, at the suggestion of Mr. Crum, that the specimens should be returned to Glasgow, and become the property of the Philosophical Society.

Mr. Bryce gave notice that at next meeting he would move that the Society take some active step with regard to the necessity of expediting the Ordnance Survey of Scotland.

A paper was read on Apiine by Dr. Adolph v. Planta (Reichenau,) and Mr. William Wallace, communicated by F. Penny, Ph. D, which has been since printed in the Philosophical Magazine.

January 8th, 1851.—*The* Vice-President *in the Chair.*

Messrs. Thomas Sheriff, Thomas Bayne, Archibald Nairn, and John M'Donald were admitted members.

Mr. Bryce's motion for a grant of £15 to the Clydesdale Naturalists' Association was voted on a second time, and finally agreed to.

Mr. Bryce brought forward the motion of which he gave notice at last meeting, "That the Society authorize and do hereby recommend the council of the Society to co-operate, as speedily as possible, with the Royal Society of Edinburgh, in urging upon the government the necessity and importance of expediting the Ordnance Survey of Scotland."

The motion was seconded by Dr. Walker Arnott.

Mr. William Brown proposed, as an addition to the motion, that the council should have a discretionary power to make a direct application to government on behalf of the object in view, and also to co-operate with any public body in the west of Scotland whom it might influence to take up this matter.

Mr. Bryce having consented to this addition being made to his motion, it was unanimously agreed to.

A letter from the Geological Society of London was read, acknowledging receipt of the last Number of the Society's printed proceedings.

Dr. Robert D. Thomson read a notice of the travels of Dr. Thomas Thomson, jun., in Sikkim Himalaya, and the Khasya hills.

Dr. Thomson exhibited likewise Boutigny's experiment of plunging the fingers into melted lead.

January 22, 1851.—*The* VICE-PRESIDENT *in the Chair.*

MESSRS. ARCHIBALD M'LAREN and William M'Adam were admitted members.

Dr. Robert D. Thomson stated that the council had this evening had under consideration a proposal which it agreed to recommend to the Society, namely, that the Society should submit to the Town Council the propriety of a weekly or monthly publication of the bills of mortality, and returns of the state of disease in the city, in conjunction with meteorological reports for the periods embraced in the returns. He moved that the Society shall respectfully recommend the proposal to the Town Council.

Dr. Walker Arnott seconded the motion, and proposed that a committee of the Society shall be appointed to communicate the suggestion to the Town Council, the committee to consist of Dr. R. D. Thomson, convener, Dr. Strang the city chamberlain, Mr. William Murray, chairman of the Statistical Section, and Mr. Keddie, one of the Secretaries. Which motion was unanimously agreed to.

A letter from the Secretary to the Royal Scottish Society of Arts was read, acknowledging receipt of the last Part of the printed proceedings of the Society.

Professor William Thomson read a paper on the question, "Is Heat Matter?" maintaining the negative opinion of the subject.

Professor William Thomson afterwards exhibited and described a series of Magnets, of which the magnetism was latent.

February 5th, 1851.—*The* VICE-PRESIDENT *in the Chair.*

MR. WILLIAM MURRAY reported that the Town Council had favourably entertained the proposal submitted by this Society, for a more frequent publication of the bills of mortality and the vital statistics of the city.

The following report was received from the Botanical Section:—

Botanical Section.

The Botanical Section met on the evening of Wednesday the 29th January, and elected its office-bearers for the year, viz.:—

President.....DR. WALKER ARNOTT.

Vice-President.....MR. WM. GOURLIE. | *Secretary*.....MR. W. KEDDIE.
Curator of Herbarium..MR. F. ADAMSON. | *Treasurer*.....MR. JOHN ALEXANDER.

Mr. Gourlie exhibited a specimen of Isonandra Gutta, the Gutta Percha plant, from Singapore.

The following paper was read:—

XIX.—*Notice of the species of Salvadora.* By G. A. Walker Arnott, LL.D.

The genus *Salvadora* possesses a peculiar interest, from its having been generally agreed of late years that one of the species is the σινάπι, or mustard plant of Scripture. The proof of this hypothesis being correct, was first analysed by Dr. Royle. (See the Gardener's Chronicle for 1844, p. 199; Athenæum for 1844, p. 272; and Kitto's Biblical Cyclopædia, ii. p. 772).

Botanically speaking, the genus was proposed by M. Garcin of Neufchatel for a plant found near the shores of the Persian Gulf, and described in the Philosophical Transactions for 1749. Although the calyx was omitted, and the corolla mistaken for it, the original description was sufficiently accurate to enable Linnæus, and afterwards Vahl, to refer plants from East India and Arabia to it. Of the old descriptions, the most correct is that given by Vahl. He considers the Persian plant, the East Indian, and the Arabian, (mentioned by Forskaol under the name of *Cissus arborea,*) to be one and the same species, and retains the name of *S. persica* given by Garcin.

The first figures of *Salvadora* I find are in Lamarck's Illustrations des Genres, t. 81, and Vahl's Symbolæ Botanicæ, t. 4, both published about the same time, and both representing the common East Indian plant. Then follows the figure in Roxburgh's Coromandel Plants, t. 26, and this of course also exhibits the East Indian form. Of the original Persian plant no figure exists, nor is there any of the Arabian one, or, which is probably the same, of an Egyptian species; for the latter, the figure in Bruce's Travels, v. t. 12, of a tree he calls Rack, is quoted by Delile in his Flore d'Egypte; but Bruce's plant is unquestionably a species of *Avicennia,* as was long ago indicated by Brown in his Prodromus Flor. Nov. Holl, p. 519.

To the *S. persica* Loureiro added two species from Cochin-China, viz., *S. capitulata* and *S. biflora;* but as these two are destitute of a corolla, have alternate rough serrated leaves, and flowers at the extremity of a long axillary peduncle, they can have no affinity with *Salvadora,* or any allied genus; what they may be I do not here conjecture.

The next addition to the genus was made by A. Sprengel, but the plant he describes, from Surinam, belongs to the Myrsinaceæ, and is the *Weigeltia Surinamensis* of Alphonse De Candolle.

Thus, then, all the forms mentioned, that really belong to the genus, have been reduced to *S. persica* of authors; but this species may possibly be made up of several, and in that case, in order to determine the true mustard tree of the Bible, it is necessary to ascertain which is the species found in Palestine.

The first, so far as I know, who indicated two positively distinct species of the genus, was Dr. Royle, in his work on Himalayan plants. He there says—"*Salvadora* is a genus common to India, Persia, and Arabia, and the same species (*S. persica*) occurs in the Circars, north of India, and

the Persian Gulf; but along with this, another species is found on the banks of the Jumna, and from Delhi to Saharumpore. This is *S. indica*, nob., *jal* of the Hindoos, *irak-hindu* of Persian authors, who also give this tree the name of *miswak*, or tooth-brush tree; the leaves are called *rasuna*, resemble those of the lanceolate Senna, and are like them of a purgative nature; the fruit is called *peel* and *pinjoo*. I know not if it be the same as that brought from Hansi, and sold in the Delhi bazaar as an edible fruit, under the name of *peeloo*. *S. persica* is called *khurjal* in northern India, *arak* and *irak* in works on materia medica. The bark of the root is acrid, and raises blisters (Roxb.); a decoction of the bark of the stem is considered tonic, and the red berries are said to be edible."

From this passage we infer that Dr. Royle considers the *S. persica* of the Persian Gulf to be the same as the plant of Roxburgh, a shrub which the latter found in the Circars, (and is not uncommon in the Peninsula and many other parts of India,) and is identical with the plants of Lamarck and Vahl, (at least the figures of both of these authors coincide with Roxburgh's;) but that there is a second species, a tree, growing along with the former on the banks of the Jumna, and about Delhi. Royle's work was published in 1839; unfortunately he gives no description of the new species more than that now quoted.

M. Jacquemont, a French naturalist, had travelled from Calcutta to the Punjaub between the years 1828 and 1832, and the materials he collected were published previous to 1844. Such is the date on the title page of the 4th volume, but this portion of the work was commenced in 1835; so that it is more than probable, as it appeared in parts, that the one containing *Salvadora* was published as early as 1839, or even previously. In this 4th volume, which is devoted to natural history, the genus *Salvadora* is introduced, the description and observations being made by Decaisne, one of the first French botanists of the present day. He there considers that only three species were known, and thus characterizes them:—1. *S. persica;* leaves ovate-lanceolate, *racemes* terminal *lax, calyx small, segments of the corolla reflexed.* 2. *S. oleoides;* leaves linear-oblong obtuse or mucronulate, *racemes short densely-flowered, calyx subcampanulate,* its segments rounded, *segments of the corolla* roundish *erect scarcely longer than the calyx, stamens included.* 3. *S. madurensis;* leaves ovate or ovate-lanceolate, racemes terminal or axillary, *calyx tubular, corolla a little longer than the calyx erect, stamens protruded.*

The *S. persica*, which he considers common to India and Persia, is thus essentially characterised from the two others, by the lobes of the corolla considerably longer than the calyx, and reflexed; in the other two the corolla is scarcely longer than the calyx, and erect; and these two again differ from each other by the calyx short (or campanulate), and the stamens included in *S. oleoides*, and the calyx tubular and stamens protruded in *S. madurensis*. The last of these, which is from Madura, an island off the north-east coast of Java, (not from northern India, as erro-

neously stated by Walpers in his Annales Botanices Systematicæ,) is unquestionably a very distinct species, as far as can be understood from the figure, for I have not seen specimens. As to *S. oleoides*, it is said to be a tree, and to form extensive woods on the western banks of the Jumna and Hyphasis, and to grow also at Khitul and Pallinlah, as well as in the salt sandy parts of the Punjaub, and to be frequent between Agra and Delhi, as far as the desert of Bihassir. Now, from this locality, and the plant being a tree, it appears to me the same as that noticed by Royle under the name of *S. indica;* and I find in my herbarium a few fragments of an unnamed *Salvadora*, collected also on the banks of the Jumna, and inserted by Dr. Wallich in his list under No. 7530, which has the narrow leaves and erect short corolla of Decaisne's plant; but Jacquemont's species is described and figured with very short (only 3-6 lines long) dense racemes or spikes of flowers, whereas my plant, which, however, is in fruit, has them from 1 to 1½ inches long, and neither crowded nor closely flowered; but in Jacquemont's, as well as in Wall. list, No. 7530, the flower or fruit is almost sessile.

Of *S. persica*, Decaisne remarks that there are a great many varieties; that sometimes the leaves are oval, sometimes linear, and occasionally both kinds are found on the same specimen: in his plant the flowers are distinctly pedicellate, giving the panicled raceme a loose appearance.

He states, as I have already said, that he only recognises three species, and yet the Arabic, or at least the Egyptian species, with which he must be well acquainted, does not quite agree with any, unless we are to suppose that he intends to combine all the forms with a reflexed corolla into one.

I now come to two works published simultaneously within these few months by my friend Dr. Wight at Madras. I allude to the last part of vol. 2 of his Illustrations of Indian Botany, and the last part of vol. 4 of his Icones. In the former a plant is figured at t. 181, under the name of *S. indica;* in the latter another at t. 1621, under the name of *S. persica*, with a portion of a third species called *S. Stocksii;* these three being all the species of the genus which he acknowledges. The first, which he says is common in India, has the corolla reflexed, leaves elliptic-lanceolate, and the racemes lax; it is thus identical with the *S. persica* of all previous writers on Indian botany, Roxburgh and Royle inclusive. The name of *indica* is, no doubt, adopted from Royle, but is certainly not the plant from the Jumna meant by the latter. The second, *S. persica* of Wight, has narrow leaves precisely as in Royle's *S. indica*, or Decaisne's *S. oleoides*, and like it has the flowers nearly sessile; the racemes, too, are as compact as in Wall. list, No. 7530, to which I have referred, but the corolla and protruded stamens are precisely as in the preceding species. His third species has the leaves ovate or oval and mucronate, the racemes compact, flowers short-pedicelled, and the corolla is said to be deciduous, while it certainly remains on till the fruit advances to maturity in all the other species. As Dr. Wight has not seen the corolla, it is

probable that it merely escaped notice from being erect and short, as in *S. oleoides*; and if so, the only distinction between this last and Dr. Wight's new species rests on the broad or narrow leaves.

Now the question again arises, what is the true *S. persica?* and which of the Indian ones approaches it most? We have two principal competitors for the honour, but before assigning the preference to either, we must examine what Garcin himself says on the subject:—

1st. The corolla which he describes in place of the calyx is "divided in four lobes, which, as soon as they spread open, turn outward, and roll backward on themselves;" the real calyx was probably small and concealed by the revolute corolla. 2d. The stamens are about the same length as the lobes of the corolla. 3d. "This plant is woody; it grows sometimes into a tree, sometimes into a shrub, and sometimes into a bush;" "that of a larger sort of a shrub is what it most frequently grows into; it produces a number of boughs without order, and very tufted branches, which most commonly hang down to the ground." 4th. The length of the leaves, which varies on the same branch, "is generally from one inch and a-half to two inches and a-half, and their width is from nine lines to an inch a little below the middle in each, which is the widest part." 5th. The flowers "are small, and disposed in clusters on the tops of the shoots." 6th. "The berry is of the shape and size of a gooseberry, of three or four lines in diameter: at first it is of a pale green, then a bright purple, and in its maturity of a dark red; each berry is supported on a strong thick pedicle attached to a small bunch." The seed "is as large as a grain of hemp seed, that is, about two lines in diameter, but sometimes less." Garcin adds, "it delights in the hottest and driest places, such as those adjacent to the Persian Gulf, and perhaps more so than palm trees, wherefore I doubt of there being any growing in the countries that lie to the east of the gulf, and accordingly I have met with none, either in the neighbourhood of Surat or in the kingdom of Bengal, where there are regular rainy seasons every year. I should rather believe it is more likely to be found in the deserts of Africa, on this side of our tropic, these being proper places for it, and where it rains seldomer than in any other part of the globe." The inhabitants of the gulf call the plant *Tchuch.*

Now, all the Indian species of *Salvadora* may be divided into those with short erect, and those with reflexed lobes to the corolla; and if, as I conjecture, *S. Stocksii* of Wight belongs to the former (Dr. Wight having seen it only in fruit, in which state the corolla of *S. oleoides* is also scarcely perceptible), then the only Indian ones that can be compared with Garcin's plants are the *S. persica* of Wight, and the *S. persica* of Roxburgh. As to the former, Wight describes it with "narrow elliptic-lanceolate leaves," but figures it with leaves linear-oblong, or narrow oblong-lanceolate, (the largest being 2½ inches long and only 4½ lines wide,) which does not accord with the original species of the genus. Again, Wight says, "the berries of the Persian plant are described as yellow or black, those of the Indian one are red," while he himself does not know the colour of

the fruit of his own *persica*. Assuredly Garcin does not say that the berries are not red, but that they pass from green to purple, and then to red. Wight seems to have copied the colours from Forskaol, as quoted in Roemer and Schultes' Systema; and Irby and Mangles, in their travels between the southern extremity of the Dead Sea and Kerak, met with probably the same bearing a fruit resembling the currant in appearance, but with the colour of a plum. As the colour seems to depend on the maturity of the berries, it is not perhaps of much consequence. Wight says of his plant that the flowers are sessile, and the panicles terminal and compact. To this there is nothing opposing in Garcin's description, although it is not so precise as could be wished. If, however, any dependance is to be placed on the shape of the foliage, on which Dr. Wight relies, his plant cannot be *precisely* the original *S. persica*.

Again, as to Roxburgh's species, the usual form of the leaves perfectly accords with Garcin's description; so that, as far as can be determined from the foliage, the presumption is strong that the species widely distributed throughout India is the same as that from the Gulf of Persia; and confirmatory of this view I may add, that the name *khurjal*, given to this species in the north of India, is almost the same as *khardal* (mustard), by which latter appellation the mustard-tree of Palestine was known to the Talmudists, and is still known in the neighbourhood of Jerusalem, according to M. Ameuny. But etymologies are not much to be trusted to in the discrimination of allied species, all with similar properties; and a difficulty of some importance lies in this:—Garcin states that the fruit is 3-4 lines in diameter, whereas the fruit of all the known East Indian species is considerably smaller than that; and although no great stress can be laid on the size, still it is an element not to be entirely overlooked. In neither the *S. persica* of Roxburgh nor of Wight is the fruit larger than a grain of black pepper. Besides, in Garcin's plant the fruit is on a strong thick pedicel, the length of which is not noticed; in Roxburgh's plant the pedicel is by no means thick, but rather slender. In Wight's *S. persica*, however, it is certainly thick, at least in proportion to its length. Thus, then, Garcin's description agrees with Roxburgh's species and not with Wight's as to the leaves, but more with Wight's than with Roxburgh's as to the inflorescence, while the size of the berry accords with neither.

Hitherto I do not possess, and have not been able to see any specimens of the Persian plant, or of that from Arabia, or the one from Palestine. As to that from Arabia, however, Forskaol describes it with oblong entire thick leaves, sessile flowers, and a berry larger than a pea, and this accords very well with the Persian one. I have no doubt of the identity of these two. Again, Delile found a species in Egypt, of which he has unfortunately not given a figure or detailed description; but of the Egyptian form I have a specimen collected by Raddi, in which the leaves vary from oval to ovate, and the flowers are almost quite sessile, thus agreeing with Forskaol's description (except as to the fruit, which I have not seen). This corroborates the opinion that the plants from Persia, Arabia, Pales-

tine, and Egypt, are all the same; and, moreover, shows the accuracy of Garcin's suggestion, that his plant was more likely to be found in Egypt than in India.

The only other species which requires notice is one from Senegal, called by Zuccarini *S. paniculata*, and on it I can throw no light whatever. I am even ignorant where Zuccarini has published an account of it. Dr. Wight considers it to be the same as Roxburgh's plant, but whether he has seen Zuccarini's memoir, or judges only from the name, which is certainly more applicable to Roxburgh's than to the Arabian or Egyptian species, I have no information. Delile says of his *S. persica*, that it is found over the whole north of Africa, from Egypt to Senegal; and Guillemin states in the Dict. Class. d'Hist. Nat. that he had received specimens of *S. persica* from Le Prieur from Senegal; although, therefore, more information is required on this point, there appears to me a presumption that the Senegal species is also the Egyptian one, and consequently that the true *S. persica*, or the plant of Garcin, extends from the Persian Gulf to the west coast of Africa.

The conclusion, then, which I draw is, that the *S. persica* of the Persian Gulf and the countries to the west, and consequently the mustard-tree of Scripture, is not the plant of Kœnig, Vahl, Roxburgh, Royle, or Decaisne, which, to distinguish it, I shall call *S. Kœnigii;* but that it is very nearly allied to *S. persica* of Wight, which for the same reason may be designated *S. Wightii;* it differs chiefly from this last by the much greater breadth of the leaves, and perhaps also by the larger fruit.

Without giving any decided opinion as to what are species or what varieties, (which I can scarcely do without having more materials at my disposal,) I propose to arrange the different forms of the genus as follows:—

A. Corolla persistent; its lobes much longer than the short campanulate calyx, reflexed; stamens protruded.

1. *S. persica* (Garcin); leaves oval or ovate, racemes rather short with close nearly sessile flowers.—Cissus arborea *Forsk.* S. paniculata *Zucc.?* —Hab. Persian Gulf, Arabia, Palestine, and northern Africa.

2. *S. Wightii;* leaves linear-oblong or narrow oblong-lanceolate, racemes rather short with close nearly sessile flowers. S. persica *Wight Icones, t.* 1621.—Hab. Scinde.

3. *S. Kœnigii;* leaves from linear to ovate, racemes lax, flowers distinctly pedicellate.—*S.* persica *Vahl Symb. t.* 4; *Lam. Ill. t.* 81; *Roxb. Cor. t.* 26; *Royle; Decaisne in Jacquem. Voy.* iv. *t.* 144, *f.* S. indica, *Wight Ill. t.* 181. Embelia grossularioides *Kœn.* Embelia Burmanni *Retz. Obs. Bot. fasc.* 4. *p.* 23. Rivina paniculata *Linn.*—Hab. From near Cape Comorin, at the south extremity of the peninsula of India, to the northern Circars, and thence to Delhi and northern India.

B. Lobes of the corolla short and erect (or deciduous?); calyx campanulate.

4. *S. Stocksii* (Wight); leaves oval or ovate, racemes rather short, with shortly pedicellate flowers.—*Wight Icones, t.* 1621. B.—Hab. Scinde.

5. *S. oleoides* (Decaisne); leaves linear or narrow oblong-lanceolate, racemes short, flowers close sessile, stamens included.—*Jacquem. Voy.* iv. *t.* 144. S. indica *Royle. Wall. L. No.* 7530.—Hab. Banks of the Jumna and northern India.

C. Lobes of the calyx erect, scarcely longer than the tubular calyx; stamens slightly protruded.

6. *S. madurensis* (Decaisne).—*Jacquem. Voy.* iv. *t.* 144.—Hab. Island of Madura, near Java.

I am unwilling to enter upon the affinities of this genus; the most probable conjectures are those lately made by M. Planchon, that *Salvadora*, *Dobera* (to which he refers *Schizocalyx coriaceus* Hochst, and *Blackburnia oppositifolia*, or rather *B. monadelpha*, Roxb., and *Azima*, to which *Monetia* of L'Heritier and *Actegeton* of Blume are reducible), form a small order allied to Oleaceæ: at the same time the distinct petals and hypogynous stamens of the two last of these genera, and the one-celled ovary of the first, are considerably at variance; while between each other they have few common links, except the structure of the seed and number of stamens.

Messrs. Wm. Black and Jas. Murdoch, jun., were admitted members.

A new drainage level, patented by Mr. T. R. Gardner, Buchanan-Street, was exhibited, and its construction explained by Mr. Bryce.

The following paper was read:—

XX.—*On Copper Sheathing, and the probable cause of its deterioration.*

By JAMES NAPIER, Esq. F.C.S

THE objects for which ships and other sea-going vessels are covered with metal are twofold. 1st. To prevent worms and other marine animals boring into the wood and destroying the vessel; and, 2d, To prevent the adhesion of sea-weed and shell-fish to the bottom of the vessel, which greatly impedes her progress and otherwise affects her sailing qualities.

The protecting of sea-going vessels, by covering them over with metal, appears to have been practised by the ancients. Leo Baptista Alberte observed upon the remnants of a ship discovered in the neighbourhood of Lake Roccia, that it had been sheathed with an alloy of copper. More commonly vessels were covered over with wood; but when the milling of lead was invented, about the year 1670, a patent was granted to the inventor for sheathing vessels with milled lead, and this was practised for a period of thirty years, but was afterwards given up and wood again adopted. The reasons for abandoning the use of lead were the fouling of the vessel, and the destruction of her iron work. Notwithstanding these results, several patents have been since taken out for lead sheathing, and the government, so late as 1832, made a trial of it on a hulk, but the lead dropped off by the rapid destruction of the nails which fastened it.

The object required is a metal or alloy that will combine the qualities of keeping the vessel clean, and protecting it from destruction by worms at the same time within a reasonable cost. Notwithstanding many suggestions and trials with different metals and alloys, none has been found more suitable than good copper.

Sheathing with copper was first adopted in the navy in 1761. In the course of forty years after, it was observed that there was some diversity in the wear of the sheathing, especially in that which had been more recently applied, which diversity has continued greatly to increase, and, according to observations made, this has become much more so since about 1832. The extent of the diversity may be stated to range from thirty months to thirty years, and this under all the varied circumstances in which a vessel may be placed. The natural idea suggesting itself to all observers as to the cause of this, was impurity in the copper used, and that the wear was in proportion to its quality. This was put to the test of experiment in 1824. At the commencement of his researches, Sir H. Davy found that sea water acted more rapidly upon pure copper than when slightly alloyed. It is to be regretted that the nature of the alloy experimented upon was not given; however, later experiments made upon sheathing which had been in wear, some remarkable for their durability, others for their rapid decay, he found their action upon sea water to differ so little as induced him to look for other causes of destruction than the quality of the metal, and he conceived it probable to be owing to the electrical condition of the metals in relation to the sea water, under certain circumstances which had to be sought for by experiment. If it depended wholly upon the electrical condition of sea water and copper, or, in other words, upon the solubility of copper in sea water, then as pure copper had been found more soluble in sea water than impure, it would consequently follow that pure metal was not the best for sheathing; therefore the conditions which regulated the electrical relations of the water and metal upon a ship's bottom became the object of Sir H. Davy's inquiry, and this being of great importance to the present inquiry, I may here briefly state what is meant by the electrical conditions of metals and solvents. If any two metals be put into a solvent, say an acid, this acid will act more rapidly upon one of these than upon the other; if the two metals while in the solvent be made to touch each other, the one which had been least acted upon will now be dissolved with less, and the other with greater facility. Thus, suppose a piece of copper in sea water be acted upon with a force equal to one, and a piece of iron in the same water acted upon with a force equal to three; if the two metals are brought into contact, the copper will cease to be acted upon, while the iron will now be dissolved with a force equal to four. The copper in this experiment is said to be rendered electro-negative, the iron electro-positive.

Sir H. Davy found that oxide of copper is negative to metallic copper. An alloy of tin and copper is negative to pure copper, and hammered or *hard copper* is negative to soft copper. That the green rust which forms

upon copper is negative to metallic copper, that the nails used were negative to the sheathing, and also that copper alloyed with small quantities of tin, zinc, iron, or arsenic, promotes the formation of insoluble compounds upon the surface of copper in sea water, and that being negative, hastens the destruction of the sheathing. Here was ample source for explaining the diversity of wear in sheathing; and, it may be observed, it brings us back to the original idea, that impure copper causes more rapid destruction when used as sheathing than pure; and although the direct action of sea water be a little greater upon the latter, it being regular and not subject to local galvanic influence, it will be more lasting, and also more effective for prevention of fouling, &c.

To overcome the evils both of the local and total destruction of sheathing by electrical conditions, Sir H. Davy suggested the rendering of the whole surface of the sheathing negative, and destroying all local electrical influences, by bringing a positive metal into contact with it. The trials and experiments made upon these suggestions were eminently successful. "When a piece of zinc having a surface equal to $\frac{1}{130}$th of the copper was attached, there was no corrosion or decay; with smaller quantities, such as from $\frac{1}{180}$th to $\frac{1}{400}$th, the copper underwent a loss of weight which was greater in proportion as the copper was smaller." Trials were made upon an extensive scale with vessels under various circumstances, and all proved the correctness of the principle. But it was found that copper thus protected soon became covered with an earthy coating, composed of carbonate of lime and hydrate and carbonate of magnesia, and to this coating weeds and shell-fish easily adhered and produced fouling, so that this beautiful application of a philosophic principle had to be, in less than two years, completely abandoned.

The late Professor Daniell, thinking that the abandonment of the principle was too absolute and premature, proposed a mode of partial protection, by arranging the protectors in such a manner that they could be withdrawn in part or in whole, and that the sheathing might be fully protected where there was no liability to foul, and where this liability existed, the protectors would be partly or wholly removed. We are not aware if trials were made upon this suggestion. The subject of fouling, and its prevention by copper, I may mention, have been viewed differently. Some suppose it to be owing to the poisonous quality of the copper compound formed by the action of the sea water, which kills the barnacles and other organisms that attempt to adhere and thus drop off, while others, and among whom ranked Sir H. Davy, consider that the weeds and shell-fish are prevented from adhering by losing their hold from the corrosion of the surface.

The next who drew public attention to this subject, was Mr. Prideaux, chemist at the Plymouth dock-yards, in a paper to the British Association at their meeting in 1841. In this paper we find it stated that the popular opinion then was, that alloyed copper was best, originating, says that authority, "from observations made upon several samples of copper

used for sheathing, especially one analysed by Sir H. Davy that lasted long, and contained 1·4 per cent. of tin, and another by R. Phillips, which only lasted four years, and was the purest copper he had seen; it was therefore considered that pure copper was not the best for sheathing, and that the presence of tin and zinc were favourable to its durability." These conclusions, be it observed, differ from that Sir H. Davy came to, and that which R. Phillips analysed may be corroborative of Davy's opinion, because such copper put upon a vessel in connection with impure or alloyed sheets will cause its rapid destruction, and all the conditions not being given, the conclusion come to from an analysis may be erroneous. Upon these conclusions Mr. Prideaux remarks, that the durability of the sheathing does not depend so much upon the presence of these two metals, tin and zinc, but that their presence guarantees the absence of suboxide of copper, which he considers very injurious to sheathing, facilitating the action of sea water both mechanically and chemically. Here we find the presence of other metals even to 1·5 per cent. thought of no consequence, except as a negative test for the presence of a compound *supposed* to act deleteriously, which, I think, is sufficiently answered by the analyses given by Mr. Prideaux of five different coppers, where it will be seen that the presence of tin and zinc are greatest in the copper most rapidly destroyed:—

	New copper.	In wear 30 yrs.	In wear 17 yrs.	In wear 5 yrs.	Rapid wear.
Tin,	—	0·08	0·07	0·10	0·07
Zinc,	0·17	0·09	0·14	0·21	0·15
Iron,	0·16	0·07	0·26	0·13	0·36
Silver,	0·13	0·01	0·14	0·01	0·06
Lead,	trace	trace	—	—	trace
	0·46	0·25	0·61	0·53	0·64

It is to be regretted that Mr. Prideaux did not give the amount of copper also in these analyses.

Pieces of each of these coppers were put into sea water having a little salammoniac in it, and exposed for twelve days, when the loss was as under:—

New.	30 years.	17 years.	5 years.	Rapid wear.
5·1	5·7	5·	4·6	5·2

Although there is not much to be deduced from these experiments, as they would require repetition, and without salammoniac in the solution, nevertheless the results correspond with Sir H. Davy's views. That which had lasted thirty years is the most pure, and sea water has the greatest action upon it.

Mr. Prideaux also examined the effects of the nails used, and says, in some cases they seemed to have acted as protectors to the copper, it being thickest round them; in other cases as negative, the copper being destroyed round them. When tried by a galvanometer the nails were found mostly negative to copper, but when the nails were covered with verdigris and

the copper clean, they were positive. It is to be regretted that an analysis of these nails experimented upon was not given, the omission of which takes from the value of the experiment. I here give the analyses of three qualities of sheathing nails, which may be taken as the general character of the nails now in use for copper sheathing. The first two are by Dr. Percy, taken from the Chemical Gazette for 1850:—

No. 1.		No. 2.	
Copper,.....	52·73	Copper,.....	62·62
Zinc,.....	41·18	Zinc,.....	24·64
Lead,.....	4·72	Lead,.....	8·69
Tin,.....	—	Tin,.....	2·64
	98·63		98·59

No. 1 is said to have corroded rapidly, becoming rotten at the heads and breaking off; No. 2 is good, and had been taken from a ship's bottom after a voyage to India and back.

The next, No. 3, are sample nails exhibited at the meeting of the British Association, Swansea, analysed by Mr. John Cameron:—

	No. 3.
Copper,.....	60·0
Zinc,.....	34·8
Lead,.....	0·7
Tin,.....	3·8
Iron,.....	0·3
	99·6

The application of any of these nails to fasten copper is a very questionable practice.

About two years ago, Mr. Prideaux resumed the subject of inquiry into the causes affecting copper sheathing, in a series of papers to the Mining Journal, in which it appears that little or no advance has been made to our knowledge of this subject since his former communication to the British Association. In these letters he says, "With respect to the quality of the metal, I have been called upon to analyse many specimens of good and bad wearing sheathing, old and recent, and to examine a great many more, and have not found in the analyses any characteristic or constant difference between the bad and the good, nor have those which wasted quickest, nor wore worst at sea, been uniformly or decidedly more susceptible to corrosive agency in the laboratory than the very best old samples."

How valuable would a table of these analyses have been, to enable others to draw conclusions, as very often men employed constantly in any particular branch investigate with certain preconceived expectations, which cause them to overlook many important circumstances; hence we find Mr. Prideaux expecting the same kind of results in the laboratory

as on the vessel, and with this view he adds, "I have from twenty to thirty samples, distinguished for good or bad sea wear, fixed to a buoy in the tide-way under exactly similar conditions, and when these come to be stripped off, if the greater or less waste correspond to their previous sea wear, it may be then fairly referred to quality of the metal, and will form a more trustworthy ground for analytical inquiry." The results of these trials with an analysis of each specimen, I hope, will yet be given to the world.

A trial somewhat similar was made at the request of Dr. Percy, by Captain James. Specimens of different coppers were kept in sea water for nine months, the loss of each per square inch is given thus:—

Electrotype copper lost,	1·4
Copper with arsenic,	1·2
Copper with phosphorus,	none.
Specimen copper marked "from Frolic,"	1·12
Copper (suppose cementing),	0·8
Copper from dock-yard,	1·66
Do. do.	3·
Do. do.	2·48
Do. do.	2·33
Yellow metal (Muntz's),	0·95

Here again want of careful analysis of every specimen, and particulars of condition, render this otherwise interesting experiment useless as data for a proper investigation; however, the object of the experiment was no doubt gained by the comparing of alloys of copper and phosphorus with ordinary metals. The results are interesting, and may be usefully applied. The analysis of such an alloy is given in the same paper, whether the exact one subjected to the above experiment is not mentioned—

Copper,	95·72
Iron,	2·41
Phosphorus,	2·41
	100·54

Experiments from which we are to deduce an application to such purposes as sheathing, may lead to false results, not being in accordance with the conditions of application, such as where one sheet of copper overlaps another, making a connection extending over the whole external surface of a ship, and embracing thousands of plates. Where a slight variation in the composition of a few will induce an electrical action throughout the whole, and thus give results entirely different to suspending any single sheet, so that we must make our experiments under the same condition, or have a thorough understanding of how to apply the results got from single sheets to the conditions to which these may be applied, such as the clear conceptions which characterises Sir H. Davy's inquiry, and from which I have no hesitation in saying, that were a ship

sheathed with a mixture of all these coppers given in Captain James' experiments, that the few sheets of Dr. Percy's alloy of copper and phosphorus would induce a rapid waste of the whole, and to analyse those sheets destroyed first and those wearing best, we should have the conclusion that impure copper is best for sheathing.

Mr. Prideaux, whose long experience in the matter under discussion deserves deference, seems almost inclined to abandon the quality of the metal, and seek the cause wholly in the conditions, which he states thus:—

1st. *Friction* from heavy shore work, faster sailing and more active service.

2d. *Corrosive waters*, as the drainage of mines, manufactures, sewers, and putrescent matters in the sea.

3d. CLIMATE—corrosive action being increased by heat, and sheathing is known to waste quicker in tropical climates.

4th. WEATHER.—Electrical and thundery, storms, &c.

5th. ELECTRO-CHEMICAL.—Nails and metal giving a positive tendency to waste.

6th. *Matters laid under sheathing*—as tar, paper, felt, which may have acid or alkaline properties.

7th. *Timber of the vessel*—some wood having acid properties, &c.

Some of these seem as catching at straws, while others, as already referred to, are important. Sir H. Davy found, that on a vessel going at a speed of eight miles an hour, the copper most exposed to the friction of the sea lost more than double that which was least exposed; and Mr. Prideaux found that pieces of the same quality of copper put into sea water from different localities, were differently acted upon. In thirteen days' exposure the waste of copper in water from

Heart of Gulf stream was.....	1·81
Caribbean Sea.....	0·40
Plymouth harbour.....	0·31

Such circumstances as these are easily defined; but the circumstances, that when two vessels are sheathed at one time, and kept nearly under the same conditions, the copper of the one lasting two or three times that of the other, or even one vessel, her sheathing at one time lasting seventeen or twenty years, and at another not more than three or four, and employed on the same service, are not so easily accounted for, and require a more strict investigation.

Mr. P. sums up his inquiry with the following:—"To whatever extent the recently increased waste of sheathing may be due, such as constant employ, much greater velocity, &c., there is reason to fear the fault is still to be sought too often in the copper itself." These views induced him to seek information, in a series of letters to the Mining Journal, to find if any modification or change had taken place in the smelting of the ores, so that it might lead to the cause of the increased deterioration of the

copper, but such information is not to be had, except by a detailed history of all the operations of smelting during the last eighty years.

Having thus briefly given an outline of the present state of our knowledge of the important question of copper sheathing, I will now call the attention of the Society for a short time to my own views of the matter, or rather to a vindication of the principle upon which Sir H. Davy based his opinion, namely, that pure copper, and uniformity of composition and character, are what are required for good sheathing, referring at the same time to some of those prominent changes which have taken place in the production of the copper, to cause the great deterioration recently so much complained of.

That old sheathing, such as that in use last century, is superior to that of this century, especially to that made within these last twenty-five years, is a fact generally admitted. Is, then, the cause of this difference due to the quality of the metal? In the absence of chemical analyses of old sheathing, I have sought out probable proof in respect to its quality in the source from which the copper was obtained. Dr. Black, in his chemical lectures (vol. ii. p. 647) says, "Anglesea contains the richest bed of copper perhaps in the world, and of late years yields about 25,000 tons of metal annually. The vein is about seventy feet thick."

These mines were discovered about the time sheathing was introduced into the navy, and it is computed that for many years not less than 80,000 tons of ore were extracted annually, and the copper commanded the market of the world. Now the copper from these mines has always been, as it still is, although the quantities now got are very small, the best and purest in quality, and entirely free from those impurities which I consider deteriorates the copper of this century. Towards the close of last century, these mines became poorer, and have gradually declined; the ores from Cornwall and other sources have increased, but the Cornish ores do not yield copper of the same purity as the Anglesea ores. The produce of the Cornish mines from 1800 to 1830 was more than doubled —that of 1800 being 5,187, and that of 1830, 11,554 tons, but considerable importations of good copper ore were made from Russia, and assisted to take the place of the declining supply of the Anglesea ores.

In so far, then, as these ores varied in quantity and quality, so would be the relative deterioration of the metal; but it was more than relative as regards the sheathing, for the superior quality of Pary's mine, and Russian copper, caused it to be used either wholly, or mixed with the best Cornish for hammered and other particular work, throwing the burden of the inferior copper into sheets, as a lower quality of copper will roll better than it will hammer.

Mr. Prideaux, to whose papers I am indebted for many valuable practical hints, asks, in one of his inquiries respecting the mixture of the ores for smelting, "Were these mixtures not modified to suit the rich American ores, when these were introduced, from which period some of the best informed persons date the most rapid sea waste in the sheathing?"

The ores referred to are from Chili, and the localities on that coast. There are some of these ores very pure, but the following analyses of two samples will show their general character:—

Copper,.....	30·6	Copper,.....	28·50
Sulphur,.....	29·3	Iron,.....	25·83
Iron,.....	21·4	Sulphur,.....	23·70
Siliceous matter,.....	16·8	Silver,.....	0·06
Antimony,.....	1·6	Silica,.....	18·70
	99·7	Antimony and arsenic,.....	2·80
A. THOMAS.		JOHN CAMERON.	99·59

Poorer ores of Chili, and which would not pay transit, undergo an operation of calcining and fusing near the mines which takes away the matrix, and the product is brought to this country under the name of *regulus*. The following two analyses will give an idea of the general composition of this compound:—

Copper,.....	59·6	Copper,.....	52·8
Sulphur,.....	19·1	Sulphur,.....	20·3
Iron,.....	15·4	Iron,.....	18·6
Antimony,.....	1·2	Silver,.....	0·1
Siliceous matter,.....	2·8	Antimony,.....	1·4
	98·1	Silica,.....	4·2
			97·4*

The ores and regulus are mixed with the Cornish ores during their progress of smelting. Previous to the introduction of these ores, the average of the ores smelted did not exceed eight per cent. The operations of smelting are a series of calcining, fusings, and roastings, amounting to about seven or eight operations, during which the greater portion of impurities are scorified. The introduction of these richer ores shortened and lessened the number of operations, and also the chance of so completely slagging off the deleterious matters. About the same time these South American ores were introduced, Muntz's yellow metal came into use, an alloy of two equivalents of copper and one of zinc. The success of working this alloy depends much upon the purity of the copper used; hence, with an increased supply of impure ores, came an increased demand for good quality of copper to make this alloy, which copper was consequently taken out of the copper market. This was obtained by the process termed *selecting*, and to show the bearing of these circumstances upon the subject under consideration, I must briefly describe the process and principle of smelting and selecting. The ore is first calcined by being placed on the floor of a large high roofed reverberatory

* Some of the iron existed as oxide, which accounts for the loss.

furnace, and kept at a dull red heat for several hours, which expels a great quantity of the sulphur, and oxidates a portion of the iron. It is then fused in a separate furnace, the silica and oxide of iron combining forms scoria, or slag; the copper with iron and sulphur combines, forming what I have described as regulus; the slag or scoria floats and is skimmed off, the regulus is tapped into a deep pit of water which granulates it. This granulated regulus is again subjected to calcining and fusing, until the iron is mostly all oxydised, when the copper remains as a sub-sulphuret, with a little iron and a portion of the impure metals. This product is now *roasted*, by being put into a reverberatory furnace furnished with air holes, and kept at a *semi-fluid* state, with a free current of air passing over the surface. The reaction may be thus explained. A portion of sulphur is carried off by the oxygen of the air, and the copper is oxidated, and this oxide of copper instantly reacts upon or is decomposed by another portion of sub-sulphuret, the copper of both being reduced to the metallic state without any carbonaceous matter. Copper in the fused state has a stronger attraction for sulphur than any of the other metals, so that when copper begins to be reduced, it will first reduce all the other sulphurets present, except iron. Therefore, by carrying on this roasting until about the half of the copper is reduced, and then tapping the furnace, this reduced portion will contain all, or mostly all, the impurer metals which had existed in the regulus. The sub-sulphuret remaining is selected and reduced by itself in a separate furnace, to make pure or *select copper* for yellow metal. Thus the process of selecting affected the whole copper trade, and particularly the sheathing, for the yellow metal was not only a competing article with ordinary sheathing, but its production almost necessitated the deterioration of that against which it was to compete. The reduced copper with the impurities was taken and subjected to long roasting and refining; if the quality after that would bear rolling it was used up for sheets, if not it was sold as *tile*.

The copper trade is now almost entirely relieved from these circumstances, by the abundant supply of Australian ores, which are mostly all pure, giving copper of the best quality; however, so far as regards the past, and the question under discussion, these circumstances all tend to show that the cause of the deterioration of sheathing is impurity in the copper.

The question now occurs, what are the impurities which have thus deteriorated our copper so much? The paucity of rigid analyses of copper, and especially of that used for sheathing, prevents a positive answer being given to this question; but of the few analyses which have been made public, with one or two exceptions, it is remarkable that there is no mention made of the presence of antimony, but often of tin, and in those given by Mr. Prideaux, there are both zinc and tin. These analyses were no doubt made with the greatest care, nevertheless we cannot help thinking that the presence of this metal has been overlooked. Our reasons for thinking so are, that antimony is an ingredient in almost all

Cornish ores, and in most of the Spanish and South American ores. Out of twenty-one samples of sheet copper obtained in the market, the analyses of which I have either made or seen, not one was entirely free from antimony, and while some had only a trace, one sample had as much as one-half per cent. Only three of these contained tin, two had sulphur, and none zinc. From upwards of fifty commercial samples of ore from as many mines in Cornwall and Devonshire, not one was free from antimony, and only fourteen contained tin. However, the character of mines varies in a series of years, and the ores obtained fifty years ago from these localities may have been purer.

The absence of lead in any of the analyses given is another thing worthy of remark. Mr. Prideaux mentions that a little lead is put into the refining, but says it is only to scorify the tin, and reduce any suboxide, but it does not remain in the copper. This is not the only reason for lead being put into copper. Lead, where there is antimony, is essential to enable the copper to roll. Copper with from three to five hundred of a per cent. of antimony, would be hard and brittle without lead, but I have seen copper with 0·65 per cent. of antimony having been made tough to roll by lead, and in use as sheathing. Two analyses out of many will suffice :—

Hard copper would not roll.		Copper in sheets said to roll well.	
Copper,	99·40	Copper,	99·35
Iron,	0·10	Iron,	·08
Antimony,	0·06	Antimony,	·15
Sulphur,	trace	Lead,	·11
	99·56		99·69

Here then the presence of antimony is not only bad in itself, but it necessitates the addition of another impurity to it, and one I consider also deleterious to sheathing. The external appearance also of the sheets favours this view of the question. "Recent sheathing," says Mr. Prideaux, "is complained of as being less smooth and compact than old sheathing;" exactly what the presence of these alloys will give as they become scorified on the surface during the annealing and rolling. "Good sheathing becomes quickly covered with a thin scale or crust of green, which adheres all over, and seems to remain. Bad sheathing keeps brighter, or takes on a soft blue crust, with patches or edges of purple." These are exactly what the experiments of Sir H. Davy, and the view here taken of the subject, would lead us to expect from pure and alloyed copper.

I have also had my laboratory experiments, extending over many months, and not yet complete, hoping to find some data to follow, but so far as results have been obtained, I am still bound by the inferences drawn by Sir H. Davy. I have taken all the published analyses of sea water from different localities, and made up little quantities accordingly, and submitted *pieces of copper* of the same quality to their action. The

only conclusion yet apparent is, that chloride of magnesium is more destructive to the copper than any of the other salts found in sea water; and according to the principle of diffusion recently defined by Professor Graham, the chlorides being most easily diffused, there is an excess of chlorides over sulphates near to the mouth of large rivers, which may account for the rapid destruction of sheathing sometimes observed on vessels lying near the mouths of rivers. However, I do not give this as any explanation of the general question.

I have again taken copper alloyed with from one to two per cent. of other metals, as zinc, iron, lead, arsenic, tin, bismuth, cobalt, nickel, and antimony, and submitted them to the action of sea water. The results, so far as they have gone, only tend to verify the general question, that pure copper is acted upon more rapidly in salt water than alloyed copper. One general principle seems indicated, namely, that copper alloyed with a metal electro-positive to it, is more rapidly acted upon by sea water than when alloyed with a metal electro-negative to it, so that I would infer that a ship sheathed with copper of various qualities of alloy, however minute, will be more rapidly destroyed than if sheathed with copper of one quality. The sample alloyed with antimony was least acted upon, that with no alloy most, the proportion being as three to seven; therefore, if a vessel were sheathed with a mixture of these qualities the waste would necessarily be rapid, and an analysis of any single sheet from the vessel would not give the true cause. This, I believe, has led to much error. One sheet having worn well is analysed and found to contain an alloy, another wearing thin in two or three years is found nearly pure; hence alloyed copper is recommended, and this is no doubt the cause of the haphazard manner in which different applications have been made, to the disappointment and loss of many.

I had intended calling attention to the various patents taken out for improvements in sheathing, as an illustration of the great want of practical knowledge, and of the application of principles to such questions which often tends to give our merchants and manufacturers a very indifferent opinion of the value of science, but time will not permit. Only one of the numerous patents taken out has stood the test of experience, as I have already noticed, viz. Muntz's yellow metal; and any want of uniformity in the sheets of this alloy, also causes more rapid destruction of the sheathing.

Mr. James Thomson explained his patent apparatus for obviating priming in steam engines.

19th February, 1851.— MR. WILLIAM MURRAY *in the Chair.*

MESSRS. ARCHIBALD M. FYFE and James Milne were admitted members.

A letter was read from the Swansea Literary and Scientific Society,

acknowledging receipt of the last part of the Society's published proceedings.

Mr. James Thomson read a paper on his Patent Turbine Water Wheels.

5th March, 1851.—*The* VICE-PRESIDENT *in the Chair.*

MR. MATHIESON was admitted a member.

Dr. Robert D. Thomson produced copies of the documents prepared for the Town Council, with a view to the monthly publication of the bills of mortality and the vital statistics of the city. He stated, that on communicating with Major Graham, the Registrar General, on the subject, that gentleman had, in the most liberal manner, placed at his disposal, for the use of the medical men of Glasgow, 250 copies of a Statistical Nosology, which would be of great service in making up the returns for the Town Council. Dr. Thomson proposed that the Committee on this subject should be continued, for the purpose of endeavouring to promote an improved registration of births. The Committee was accordingly continued.

Dr. Allen Thomson read the first part of a paper "On the Structural Relations of the Nervous and Muscular Textures in the Higher and Lower Animals."

19*th March*, 1851.—*The* VICE-PRESIDENT *in the Chair.*

MESSRS. CORNELIUS J. HUGHES, J. E. Harvey, and Dr. John Aitken, were admitted members.

The following papers were read:—

XXI.—*On the Physiological Actions of Spartine and Scoparine, with a Notice of their Chemical Constitution.* By ARTHUR MITCHELL, M A., M.D., &c.

BY subjecting large numbers of plants, under circumstances as nearly similar as possible, first to one powerful chemical reagent, and then to another, we might almost predict results of considerable interest. With the view of elucidating the nature of vegetables by these means, a series of investigations was instituted by Dr. Stenhouse, of St. Bartholomew's, London, and the results, as communicated to the Royal Society, fully realize anticipations.

During these experiments, it was found that almost all plants, when subjected to the action of strong nitric acid, yielded oxalic and nitropicric acids, showing that many more vegetables are capable of yielding this

latter body, than is generally supposed. Among the plants experimented upon, and yielding these results, were the Bedford willow, the laburnum, the mahogany, the apple tree, the hawthorn, the black currant, the alder, the furze, the heather, turmeric, the alder, and the common broom.

Some peculiar exceptions, however, presented themselves:—The populus balsamifera, and the other plants of the poplar tribe, were found to yield a new acid, which has been called the nitropopulic acid, and which resembles, in several of its characters, indigotic acid. It is deposited in silky needles, in groups having a concentric arrangement. The production of this body, therefore, seems to characterize the poplar tribe, and is probably the result of the action of NO_5 on the populine they contain.

The extracts of the common oak and birch, yielded simply oxalic acid. Neither nitropicric, nor any other analogous nitrogenated acid, could be found.

I have thus given in very brief and general terms, the results of these interesting researches.

As the extract of the spartium scoparium, or common broom, besides yielding nitropicric acid, as above stated, exhibited some interesting peculiarities, it was subjected to a more minute examination.

When an aqueous decoction of this plant was concentrated to about one tenth of its bulk, and set aside in a cool situation for half a-day, it was converted into a gelatinous mass, of a greenish brown colour. This jelly was then thrown upon a filter, and washed with cold water, slightly acidulated with hydrochloric acid, and further purified by repeated crystallizations out of hot water and spirit of wine. In this pure condition it consists of pale yellow prisms, and has a feebly acid reaction. Its behaviour with chemical reagents is of a negative and doubtful character, neither distinctly occupying the position of an acid or a base. Its empyrical formula is $C_{21}\ H_{11}\ O_{10}$. To this substance Dr. Stenhouse has given the name scoparine, and with its physiological actions we shall have shortly to deal.

I now proceed to notice, with equal brevity, the chemical characters of the other substance named in the heading of the paper.

When the mother liquor from the crude scoparine has been concentrated to a moderate bulk, and distilled with an excess of soda, we obtain in small quantity at the bottom of the receiver, a pale yellow basic oil, which has been designated spartine. Its reaction is strongly alkaline, the most powerful acids being completely neutralized by it. The base itself is but slightly soluble in water, but disappears readily in alcohol and ether. Its combinations, however, with NO_5, SO_3, and HCl, are exceedingly soluble, and crystallize with great difficulty. With nitropicric acid a body is formed, in long acicular crystals, scarcely differing in appearance from the compound of that acid with potash. Its formula is, $C_{15}\ H_{13}\ N,\ C_{12}\ H_2\ N_3\ O_{13} + HO$.

The double chloride of platinum and spartine crystallises in rectangu-

lar prisms of considerable size and great lustre. This salt contains 2 equivalents of water which it loses at 266° F.

The double salt of mercury and spartine presents the form of the right rhombic prism. The crystals formed are large and of great brilliancy. The ultimate composition of the base itself, as found by analysis, gave, *as an average*, in 0·2507 grammes of spartine of CO_2 0·7052, and of HO 0·251, and its formula has been fixed to be C_{15} H_{13} N.

Between spartine and scoparine every one will have observed a wide difference. The former is a body possessing very strongly the properties of a base, readily entering into combinations, and in such cases presenting us with regular and beautiful crystalline forms; while scoparine, on the other hand, is much more of a negative, and is found wanting in positive characters. *They both exist as spartine and as scoparine in the plant, no violent chemical reagent having been employed in their extraction. The one is already prepared in a concentrated decoction of the broom, and to obtain the other nothing is needed but simple distillation, an alkali being added to liberate it from its combination.*

The results of the chemical examination of this plant are therefore highly satisfactory, and especially so when we compare them with those obtained by Cadet de Gassicourt, and given by him in the Journal de Pharmacie, (x. 448.) He tells us that broom tops are composed of "a concrete volatile oil, wax, chlorophylle, a fatty matter, a sweet substance, a yellow collouring matter, mucilage, tannin, albumen, and woody fibre." There is a vagueness about this which shows to disadvantage side by side with the precision of our knowledge respecting spartine and scoparine. Unfortunately, the chemical examination of the plants employed in medicine are too frequently like those by Cadet, and much too rarely like that the notice of which I now conclude.

We have hitherto regarded the physiological action of broom upon man and the lower animals to be that of a diuretic, occasionally producing, when given in very large doses, vomiting and purging. Its diuretic action, however, has always been looked upon as the most prominent. Notwithstanding these views, it struck me as possible that these two principles might have separate and independent physiological actions, and to determine this point I began a series of investigations. With scoparine, the gelatinous principle of broom, I commenced, and my first experiments were, of course, made on the lower animals.

Obs. I. Three young rabbits were placed under such circumstances as that all the urine voided by them in twenty-four hours could be ascertained with accuracy. They received as nearly as possible the same food, both as regards quantity and quality, and being left in this condition during two complete days, it was found that the daily amount excreted by each was pretty nearly equal. The same three rabbits were continued under the same conditions during a second period of two days; but on this occasion I gave to two of them 3 grain doses of scoparine repeated every eight hours. I found at the expiry of this period, that the two

under the drug had voided more than double the amount of urine which they had passed in the same time formerly, and double also of the amount passed by the third rabbit, which was left in the old state without receiving any of the medicine. The same three animals were kept during a third period of two days, and at the end of that period the urine they voided had reached the original standard. Before performing this experiment in so conclusive a manner, I had made repeated observations on single rabbits, where, however, the liabilities to error were considerable. Yet, although the results in this case were of a satisfactory nature, I repeated the observation, employing three other rabbits, and obtaining almost the same results.

Obs. II. I gave to a young dog, whose urinating powers I had tested as well as I possibly could, 5 grain doses of the scoparine, repeated at intervals of eight hours; and I found that, when under the drug, he invariably passed an amount of urine considerably above the standard of health. This experiment I repeated several times.

Obs. III. When I had reached this point in the research, I felt at all events satisfied that the substance was harmless in moderate quantities, and I accordingly exhibited it in 5 grain doses to A. B., a young man in good health, and leading a life having an average share of out-door exercise. In his case it had been ascertained during a lengthened train of experiments, for another object, that the average amount of urine passed daily was 34 oz., and that the widest range was only 5 oz., or from 32 to 37 oz., while at the same time it was shown that the average specific gravity was 1023. Now, how did the use of the scoparine affect these figures? The third dose had not been taken when there was an evident increase observed in the secretion from the kidneys, and this continued during the whole period of its use, and disappeared on its withdrawal. But to what extent was this increase observed? The average rose from 34 to 80 oz. daily, and at the same time the specific gravity fell to 1010. And it is worthy of note, (as confirmation of a continued diuretic action,) that during the latter period of its administration considerable thirst was experienced. This was not, however, gratified, the solids and fluids being in quantity and quality as nearly as possible the same, when the average was 34 oz., as now when the average was 80 oz. And it has still further to be noted, that a return to the former high specific gravity at once followed its discontinuance. This observation was repeated several times on the same person, and afterwards on others.*

That this is a diuretic principle, therefore, we can have no doubt. It increased the whole amount voided, lowered the specific gravity, created thirst when persevered in, and, in the case of the rabbits, rendered clear,

* Among these I mention Dr. Pereira, to whom Dr. Stenhouse sent a small quantity, and who experienced in his own person an evident diuretic action from a single 5 grain dose, the quantity voided amounting to 78 oz. He considers it a diuretic, but not a powerful one.

transparent, and acid, their urine, which is nominally opalescent and alkaline. In its action upon the kidneys in man, it appears merely to increase the secretion of water. In the case of A. B., the amount of solids thrown off daily in the urine was, on an average, 827 grains; and while under the stimulus of scoparine, the amount of solids averaged 826 grains; thus showing the singular fact, that between the periods when the whole excretion averaged 34 oz., and that when the whole averaged 80 oz., there was only the difference in the average of contained solids of 1 grain. These results are so very close, that they almost seem accidental; but it must be remembered, that in both cases the averages are struck from a long train of experiments, the only safe method of conducting such researches as the present. As far as possible, too, I had made all the conditions in the two cases identical. We thus find the absolute amount of salts in the urine to be unaffected, while the aqueous portion is more than doubled. I ascertained, too, in a rough manner, that these salts were in the relative proportions of health.

Now it seems somewhat astonishing that a body possessing such indefinite and negative properties as scoparine, should have a diuretic action, or indeed any marked action at all on the animal economy. And it at once becomes a point of interest to ascertain the channel by which it stimulates the kidneys to an increase of their secretion. About the period of conducting these researches, I happened to observe, in giving gallic acid in pretty large doses, for the purpose of arresting hæmorrhage from the lungs, that it exerted a diuretic action. A few experiments and additional observations determined the question. In examining the urine of these cases, I detected, without difficulty, the presence of gallic acid itself. The same thing was done in the milk of the mother, and the urine of the child at the breast. I believe this is the first notice of such a property possessed by gallic acid, although Dr. Christison recently told me that such has of late been observed in the Edinburgh Infirmary. I make allusion to it, however, with reference to its mode of action. This substance appears to operate as a direct stimulant to the secreting vessels of the kidney, being taken into the current of the circulation, and carried without undergoing any decomposition *in transitu* to the urinary organs. Now to this class of diuretics, Pereira refers broom; but *I* could in no case detect any evidence of the existence of scoparine in the urine. I therefore felt inclined rather to suppose that being partially acted on by the digestive organs, some of its component parts thus eliminated had been conveyed to the kidneys, and that they had thus been stimulated to increased action, *or*, that this substance had acted primarily on the stomach, and that its action on the urinary organs was a secondary one, communicated by sympathy. Finding this diuretic property in both gallic acid and scoparine, I thought it possible that some of the other yellow colouring principles might be similarly endowed. And such observations as I made were affirmative of this expectation, so that we have a class of bodies somewhat analogous in

their composition and general chemical relations, having probably analogous physiological actions. Amongst the substances referred to, I may enumerate the two yellow colouring principles from the bois jaune or morus tinctoria, the morine and moritannic acid of isomeric composition, the quercitannic acid, the purreeic acid, the morindine from the morinda citrifolia, &c. &c.

With these remarks I terminate, for the present, the consideration of scoparine, although, after pointing out the physiological actions of spartine, I shall have occasion briefly to revert to it.

Spartine.—This substance, a volatile oil, has a strong and persistent odour of tobacco. Its taste is acrid and bitter. It acts locally as an irritant. When applied to the mucous lining of the eye, it produces redness and pain; and, indeed, to whatever part applied, as, for instance, to a cut surface, it excites immediate expressions of suffering. These local effects, however, are lost in the remote action of the drug.

Obs. I. When a couple of drops of the oil were laid upon the tongue of a young dog, symptoms of uneasiness from the local irritation were at first observed, but these soon disappeared, and were followed by drowsiness and loss of muscular power, which lasted for fifteen or twenty minutes, and then gradually disappeared without producing any apparent injury.

When the dose was increased to three or four drops, and when the base was saturated with acetic acid, the same symptoms were produced, only in a more intense form. The animal made some staggering forward movements, became drowsy, and at last fell asleep. Some slight convulsive movements were observed, but they were very trifling, and are better styled tremors than convulsions. The respiration and pupils in these cases remained unaffected, at least to any observable degree.

Obs. II. When a single drop of the oil, dissolved in weak acetic acid, was given to a young rabbit, a state resembling intoxication was produced after the lapse of two or three minutes; the animal staggered in walking, allowed its head to drop upon the floor, dragged its limbs forward, opened and shut its eyes heavily and slowly, and then adjusted itself for sleep. From this state it soon recovered. When its posture was rendered uncomfortable, it invariably made efforts to rid itself of the annoyance, showing that the external senses are unimpaired or not wholly destroyed, at least while the action of the drug is only carried thus far. I was able also to satisfy myself that sight and smell were not materially, if at all, affected. I repeated this observation several times on different rabbits, increasing slightly the dose, and obtaining a corresponding increase in the intensity of the symptoms, sometimes producing a depth of narcotism and general paralysis, from which it seemed doubtful if the animal would recover. I also found, in repeating these experiments, that the substance acted more energetically when in combination, than when exhibited in its pure state.

Obs. III. In gradually increasing the dose, as detailed above, I ascer-

tained that six drops produced the death of a rabbit in a few minutes. Deep stupor, with palsy of the voluntary muscles, followed rapidly by palsy of the diaphragm, &c., terminated in death from asphyxia. Slight convulsive movements took place, and the pupil appeared to be contracted. No internal lesion was detected on examining the cadaver. No vomiting, or purging, or voiding of urine preceded death. Efforts were made to restore life to the animal, and artificial respiration was persevered in for a considerable time, without any success. Sometimes a longer period was required to produce death, and, in one case, it occurred three hours after the drug had been given.

It thus appears, in general terms, that spartine is a pretty powerful narcotic poison, producing death in doses which are small. The existence of a body possessing these properties in broom was what I had never anticipated. Yet on inquiry I find that every sheep farmer in the Highlands is familiar with the fact. And country people generally consider the plant to possess intoxicating virtues, as evidenced by its use in Germany and elsewhere in the preparation of beer, to which it is supposed to impart its heady influence. During snow storms, when the sheep are compelled to feed almost entirely on the tops of broom, it is a common thing to see them reel and give evidence of intoxication. The facts would of course have remained unaltered, whither such had previously been observed or not, but still it is satisfactory as in confirmation.*

ANALOGIES TO CONÏINE AND NICOTINE.—There are two other plants which yield volatile natural alkaloids, and to the properties of these my attention was naturally directed. I refer to conïine and nicotine, whose general chemical and physical characters present a very close similarity to those of spartine. In their ultimate composition, too, while there is certainly not a sufficient analogy to make us predict in all three the same physiological actions, yet there does exist an analogy close enough to arrest attention. There is at all events a marked absence of dissimilarity. I represent, in a tabular form, the ultimate composition of these three bodies.

Conïine, as determined by Ortigosa, ...C_{16} H_{16} N.
Nicotine, as determined by Barral,.....C_{20} H_{16} N.
Spartine, as determined by Stenhouse,.....C_{15} H_{13} N.

Now these three bodies may be said to possess the same physiological actions, differing only in the degree of intensity, the first being the most

* Dr. Paris has ingeniously referred the diuretic action of digitalis to its sedative influence. Had he known that broom possessed a narcotic principle also, he would have referred its diuretic action also to the same cause. But is it not more probable that the diuretic property of digitalis depends upon one principle, and the sedative upon another, which are separable? Dr. Paris reasons thus:—As the energy of absorption is generally in the inverse ratio of that of the circulation, it is presumable that all means which diminish arterial action must indirectly prove diuretic, by exciting the function of absorption.

powerful, two or three drops injected into the femoral vein of a dog killing it in a couple of seconds. They are all three narcotic poisons of considerable power, acting upon the spinal cord, of which they appear to exhaust the energy. They are, I think, the only three volatile natural alkaloids with which we are acquainted, at least the only ones extracted from plants employed in medicine.

I have often felt inclined to believe that bodies similarly constituted, chemically and physically, should exert similar influences on the animal economy, and, as the reverse, that where similar effects are manifested, similarly constituted principles should be found to exist. I know that serious difficulties meet me on the threshold, but some of these are melting away under more accurate modes of investigation, and the new facts which are from time to time being added to our stock, for the purposes of generalization, are rather in favour of, than opposed to, such a view.

In the datura stramonium, hyoscyamus niger, and atropa belladonna, all belonging to the solanaceæ, three principles have been discovered, atropine by Liebig, daturine and hyoscyamine by Brandes. And it has been pointed out by Runge, that these three bodies have the same composition, $C_{34}\ H_{23}\ O_6\ N$. Now between their physiological effects there exists no difference in kind. They all produce "dilatation of the pupil, insensibility of the iris to light, diminished feeling, giddiness, delirium (extravagant) followed by stupor, a remarkable affection of the throat and mouth." Between these results, however, and those from nicotine, conëiné, and spartine, we have essential differences; instead of dilatation we have contraction of the pupil; we have the absence of delirium and the throat affection; and, as somewhat characteristic, we have the paralysing effect on the muscular system. But we have not only a wide difference in their action on living beings, but a wide one also between their chemical constitutions and general properties. And, as a further illustration, it has been found, that wherever a plant has been generally and extensively employed to yield a favourite and refreshing beverage, some body has been detected in it, either the same as theine, or closely allied to it.

Most people admit, as a general rule, "that plants of the same family, or, in other words, having the same botanical affinities, agree in the nature of their medicinal operation," but we cannot, therefore, argue with Dr. Lindley, in saying, "that a knowledge of one plant is a guide to the practitioner, which enables him to substitute, *with confidence*, some other plant which is closely allied to it," for to this statement there are many remarkable exceptions, which, however, diminish, but do not destroy its utility in practice.

In what manner then does spartine produce the effects detailed? The answer may be derived in part from what has been already stated. Omitting altogether the question whither it acts by absorption or by sympathy—a questio vexata on which no new light is thrown by these researches—we are led to locate its primary action on the

spinal cord. The effects, however, are directly opposed to those of strychnine, for while the latter exalts the nervous energy of the cord, and produces muscular spasm of more or less permanence, the other exhausts it, and causes muscular paralysis. Both appear to act (but in ways opposed) on the seat of the reflex functions; and, if Grainger's views be right, this must be on the gray matter of the spinal cord. And here I am naturally brought to a practical inference. If the actions of this body are so manifestly the counterpart of those of strychnine, it follows, or is presumable, that in cases of poisoning by the latter substance, it should prove an antidote or remedy; and so also, in like manner, in convulsive or spasmodic diseases, with analogous symptoms, as tetanus, hydrophobia, &c. Now, strange to say, I find that in 1813, Marochetti recommended broom as a specific in cases of hydrophobia; and Geiger, in his Pharmaceutical Botany, tells us, that even yet the *genista tinctoria* is employed in Germany in cases of this disease. Tobacco, too, from which nicotine is prepared, enjoyed at one time the reputation of being curative of this affection; and, in 1838, a trial of conëine was actually made with such an object in the London Hospital. Of this I give a brief report:—"In the case of hydrophobia, in a middle aged man, after the disease was fully formed, two minims of conia, dissolved in 30 drops of acetic acid, were applied endermically to the pericardium. The effects were instantaneous. The pulse fell from 64 to 46, and became more regular. The vomitings and convulsions ceased; the respiration became less difficult, and the symptoms of the disease became altogether mitigated. The man expressed himself as feeling much better, and entertaining hopes of an ultimate recovery. These effects, however, were but transitory, and, in about seven minutes, the symptoms began to reappear, and shortly assumed their previous urgency. Three minims were injected into the rectum, about a quarter of an hour after the endermic application, but it produced no effect in allaying the symptoms of the disease. The remedy was not repeated, and the man became rapidly worse, and died in a few hours." Convulsive movements have been several times produced in rabbits by strychnine, and have almost invariably been stopped by conëine, but instead of preventing, it has appeared to hasten, a fatal issue. May this not have arisen from the remedy itself being too powerful an agent, too active a poison? And might we not with reason, in such a case, make trial of spartine, whose effects are identical in kind, but whose power or violence is much diminished? Under this feeling I made the following experiments:—

Obs. I. To a full grown rabbit I gave ½ grain of strychnine, which produced, in twenty minutes, violent, persistent, and general spasms. The head was rigidly bent backwards, the muscles of the abdomen were tense and hard, the limbs stiff and inflexible, and, as far as could be discovered, respiration was wholly suspended, as was also the case with the action of the heart. I placed three minims of pure spartine immediately upon the back part of its tongue. The result was striking. The spasm

and rigidity instantly and wholly disappeared, and the body of the animal became so flaccid as to appear unnatural by comparison with its previous condition. Beyond a slight and occasional pulsation of the heart, no evidence of life, however, still existed. I at once began artificial respiration, and after persevering for three minutes, a jerking and irregular respiratory movement was restored, which gradually became natural, and was followed by evidences of returning consciousness, until at length the animal was able to walk about. It still, however, appeared to be uneasy, and refused to eat. In less than ten minutes slight convulsions returned, and these went on increasing in severity, till they attained a parallel with those which first occurred. I again exhibited three drops of spartine, and again the spasm disappeared. But on this occasion I failed in establishing the respiration, and the animal became cold and died, without the occurrence of convulsions, or even twitchings.

Obs. II. To a young healthy rabbit I gave strychnine in doses of $\frac{1}{100}$ of a grain, repeated at intervals of half-an-hour. Five doses had been taken without any unpleasant effect. Shortly after the fifth, however, most violent and intense tetanic spasms or convulsions were suddenly induced. I instantly gave it three drops of the base, and as immediately all spasm vanished. The animal, however, never recovered, death taking place from the action of the spartine.

Obs. III. I gave less than one thirtieth of a grain of strychnine to a young rabbit, and no symptom of uneasiness occurred till nearly three hours after the exhibition of the drug. At this period slight and occasional twitchings were observed in the limbs, which gradually embraced the muscular system generally, and at length became violent and lasting spasms. I then gave very small doses of the spartine, dissolved in weak acetic acid, at short intervals of about two or three minutes. I continued this till I had given in all a couple of drops of the base, and as I went on I found the intensity of the spasm giving way, and the intermissions lengthening, until nothing remained but occasional twitchings, which, in their turn, as I persevered with the spartine, also disappeared, and the animal fell asleep. It remained in this condition for half-an-hour and upwards, when suddenly the breathing became laboured, and death, from asphyxia, ensued in a few minutes. There were no convulsions.

Obs. IV. Scarcely one fiftieth of a grain of strychnine was given to a young rabbit, but this was sufficient to induce, after a lapse of six hours, a state of restlessness, with slight and occasional startings of the muscles of the limbs and abdomen. A small quantity of the spartine, neutralised in weak acetic acid, was then given slowly and cautiously to the animal. Drowsiness followed, and the intervals of repose between the twitches were lengthened, but there was no sudden cessation of the spasmodic action. The animal perfectly recovered. Had no spartine been given in this case, I do not think the issue would have been fatal, but the duration and intensity of the spasms would probably have been greater.

All that I would say as the result of these experiments is, that I would think spartine worthy of a trial in tetanus or hydrophobia; but in making such a trial, my hopes of success would not be very high, for I have a resistless conviction that the diseases are scarcely amenable to treatment. Dismissing, however, the practical view, I rest the interest of these facts on their scientific or physiological aspect. *One train of symptoms is induced by one substance, and an opposite by another; and it is found that the effects of the first, when manifested by an animal, can be removed, or, so to speak, neutralised by the exhibition of the second.*

We have thus shown in broom the existence of two principles, the one diuretic and the other narcotic. In employing, therefore, a decoction of broom, as has hitherto been the practice in dropsical and other affections, we subject the patient to the narcotic influence of the spartine, as well as to the diuretic effects of the scoparine, a result which might not be desirable, but which is not of much consequence in this particular case, since the amount of spartine given in the decoction is exceedingly trifling. I have lately observed, however, that where it was administered freely, its soporific qualities were rendered sufficiently evident. To avoid these I do not think it necessary to employ chemically pure scoparine. If a simple decoction be evaporated to dryness on the water bath, then treated with a little dilute hydrochloric acid, the mixture thrown upon a filter, and washed with cold water, almost the whole of the spartine will be removed, and the dark green gelatinous mass remaining on the filter will be found to possess the diuretic without the narcotic properties of the plant.

I have said that, in the case of broom, I do not think this separation of much value, as regards the practice of medicine, and I have given my reasons. It might have been otherwise, however. We have many plants in the materia medica possessing known complex actions, depending (as I am inclined to believe) on principles of definite chemical characters, which are *separable*, and to possess these *separated* would surely be a benefit. *Thus*, we have a plant exerting a physiological action, which I shall call A B C; and we give it in a case for its action A, where its actions B and C, are contra-indicated. To say the very least, we have done a thing that was far from desirable, and we should certainly have been gainers, great gainers, had it been in our power to administer A alone, when we wanted its effects alone; and so also, in like manner, B and C alone, when we desired *their* uncomplicated effects. Regarded in this light only, such researches as the present are proved to be of importance, and of practical importance too, although in one case it may be negative, and in another positive.

I offer a further illustration of its importance. There is a very great diversity of opinion about the efficiency of broom as a diuretic. According to Dr. Pereira, it never fails to act upon the kidneys, and is the most certain of all diuretics in dropsies; and he states that he cannot call to mind a single instance in which it failed. Mead, Cullen, and Pearson.

take a middle course; while Christison and others pronounce it uncertain, and, as compared with others, of little value. Now, these researches account, in some measure, for this discrepancy. It was found that plants, which had grown under different circumstances, yielded very variable amounts of these active principles, and of all experimented on, those were found to yield most which grew on a low-lying sandy ground, with a sunny exposure, and which seemed to be stunted in their growth, the entire plants not exceeding a foot or a foot and a-half in height I need not enlarge on this point, for it is thus quite clear how one man's experience of the efficiency of the plant may be widely different from that of another. But I cannot pass this opportunity of alluding to the uncertainty that must necessarily attend the use of all vegetable infusions or decoctions where so much of their activity depends upon the circumstances under which the plants grew, &c. And, while I notice this uncertainty, I cannot but call attention to the comparative certainty that attends the employment of the vegetable alkaloids. When we give, for instance, 1 grain of quinine, we know exactly that which we have given, and we may reckon, with corresponding confidence, on its operation; but when we give of the cinchona bark what we deem an equivalent of 1 grain of quina, we are presuming some certainty where very wide variations are known to exist. And if that for which we give the bark be really the quina contained in it, I have shown it to be, in a twofold sense, advantageous to give the alkaloid itself; for, in the first place, we give it freed from such other matters as shall either impede, modify, or counteract its action; and, in the second place, we know precisely the amount of the substance given, whose action we desire.

With how great certainty can we count upon the effects of morphine, strychnine, nicotine, or any of the other active principles of plants which are employed. Among such a class of bodies, I feel firmly convinced we shall seek, with profit and wisdom, for additions to our pharmacopœia; or rather, I should say, important substitutions, since the voluminous list of vegetables at present admitted into the materia medica, would rather be diminished than increased by such discoveries. This is very evident, if we suppose (as is probable) that similar actions are dependant upon similarly constituted "active principles" existing in these plants, and which, I believe, in nine cases out of ten, to be capable of isolation.

Influenced by considerations like the foregoing, I undertook the experiments detailed. But I did not confine myself to spartine and scoparine. I investigated the actions of some other natural vegetable alkaloids, and of one artificially produced. I refer to furfurine, the base of the furfurol. I am led in these researches to suspect that this may prove an antiperiodic, and, as such, a cheaper substitute for quinine. More extended experiments, however, are required, the real modus operandi of antiperiodics being very imperfectly known.

XXII.—*On the Estimation of the Commercial Value of some Specimens of Black Oxide of Manganese.* By Mr. George William Brown.

The value of binoxide of manganese may be estimated in various ways, all of which depend on the determination of the amount of oxygen which they contain. The first of these is the method by oxalic acid, and consists in the oxygen of the manganese giving oxygen to the carbonic oxide of the oxalic acid, and converting it into carbonic acid. The first method of accomplishing this was proposed by Berthier, who disengaged the carbonic acid in a flask from which a bent tube passes into another flask containing barytes water; the carbonic acid being disengaged, passes through the barytes water, and precipitates the barytes in the form of carbonate. The precipitate is then weighed, and from the amount of oxalic acid decomposed, the amount of oxygen is calculated.

A modification of this plan was made by Dr. Thomson, who, instead of estimating the amount of oxalic acid decomposed by weighing the carbonic acid, calculated it by the loss sustained in the flask containing the oxalic acid, &c. This method is much more convenient, as one small flask is only employed, while, by Berthier's plan, two flasks are necessary. Less time is also required to calculate the value of the manganese by this plan than the former The mode of applying this method is, to fit a light flask with a cork, through which a small tube containing chloride of calcium passes. The oxide of manganese is then weighed out in the tube which is suspended from the cork by a thread; the oxalic acid, with a small portion of sulphuric acid and water, being first introduced into the flask. The whole is weighed, and then the tube containing the manganese is allowed to drop into the oxalic acid. Carbonic acid is immediately disengaged. When the action is over the apparatus is weighed again; the difference between the first and second weights gives the quantity of carbonic acid which has escaped. From this weight the oxygen in the manganese is calculated. Then, since there are two atoms of carbonic acid driven off, and the weight of two atoms of carbonic acid exactly equals the weight of one atom of binoxide of manganese, the amount of loss between the first and second weighing will be equal to the amount of binoxide in the specimen.

I have made two analyses of a specimen of sesquioxide of manganese from New Brunswick, the results of which are as follows:—

50 grains was the amount worked on.

First analysis.

Weight of apparatus before disengaging CO_2,.....	=	1105·16
Weight of apparatus after disengaging CO_2,.....	=	1077·10
MnO_2 in 50 grains,.....	=	28·06
MnO_2 per cent.,.....	=	56·12

By another method the equivalent of binoxide per cent. was found to be 55·99.

This method is very convenient, both from the short time it takes to make an experiment, and from the small number of weighings required; it also yields very correct results.

The next method is that of Gay Lussac, which is effected by chlorimetry. The manganese is weighed out and conveyed into a flask with a bent tube passing into an inverted retort, into which a certain quantity of water with a little caustic lime is introduced. Muriatic acid is then poured on the manganese, and a slight heat is applied, the chlorine is evolved, and is absorbed by the milk of lime. After all the chlorine is disengaged, the milk of lime is taken out and tested, and from the amount of chlorine, the amount of per centage of binoxide of manganese is calculated. The milk of lime containing the chlorine is then tested by the method of Gay Lussac, which consists in the conversion of arsenious into arsenic acid: and from the amount of chlorine taken to convert the arsenious into arsenic acid, the binoxide of manganese is calculated.

Instead of arsenious acid, a solution of the nitrate of the protoxide of mercury may be employed. When the chlorine is added to this solution, a precipitate of dichloride of mercury or calomel immediately falls, and as more chlorine is added, more dichloride of mercury is precipitated, till all is thrown down, when immediately on the addition of a few drops more, the whole disappears, since the dichloride of mercury is quite soluble in a slight excess of chlorine, being converted into corrosive sublimate. Whenever the liquid becomes quite clear, the process is to be stopped. As a certain quantity of nitrate has been taken, the amount of chlorine will be obtained in the quantity of liquor which it took to dissolve the chloride of mercury, and from that the quantity in the whole liquid, and thence the quantity of binoxide of manganese in the specimen.

The next method consists in the conversion of binoxide of manganese into protoxide, by reduction by hydrogen gas. The first stage to effect this, is to ascertain the amount of water contained in the specimen. The binoxide of manganese is weighed out in the bulb of a hard chloride of calcium tube, and then heated gently in a current of dry air. The air is drawn over it by an aspirating vessel, and is first passed through sulphuric acid, which dries it; it then passes over the manganese, and carries off the water, which is caught in a tube containing chloride of calcium. When all the water is driven off, the manganese is allowed to cool, and then weighed; the decrease in weight in the binoxide of manganese, and the increase in the chloride of calcium gives the quantity of water contained in the amount of manganese taken. After the water is driven off and weighed, the apparatus is then put up again, only the aspirating vessel is not required, but a flask containing zinc is fastened on at the other end. The whole apparatus being then made quite air tight, a little water and sulphuric acid are poured on the zinc so as to evolve hydrogen, which is dried by passing it through sulphuric acid, and is

then brought in contact with the binoxide of manganese, which is to be heated to redness. The manganese begins at the edges to burn; the combustion gradually approaches the centre; and when heat is applied somewhat longer, the mass becomes quite green from the formation of the protoxide. The hydrogen uniting with the oxygen of the manganese is converted into water, which is caught by the chloride of calcium. The oxide of manganese is then allowed to cool in a current of hydrogen, to prevent its oxidizing. When cool, it is weighed, and the decrease of weight between its present and the former weight, gives the quantity of oxygen above protoxide contained in the mineral. The chloride of calcium is then weighed, and the increase multiplied by $\frac{8}{9}$ gives also the quantity of oxygen, which is a check on the decrease of the binoxide. The protoxide thus formed is then converted into protosesquioxide or red oxide, by heating it in a current of air. This is again weighed, and from the increase between it and the protoxide, we can calculate the amount of oxygen that should be in the specimen, if the red oxide and protoxide were pure.

TABLE I.

Exact results of Analyses.

Binoxides.

	Amount used in grains.	Water.	Oxygen.	Impure Protoxide.
Geissen,	50	·64	8·91	40·45
— 2d example,	"	·65	8·88	40·47
Kilpatrick,	"	1·61	8·65	39·74
— 2d exam.,	"	1·52	8·54	39·68
Thuringia,	"	2·43	7·30	41·00
— 2d exam.,	"	2·02	7·14	40·84
New Zealand,	"	3·19	7·01	39·80
Table Mountain,	"	1·12	5·67	43·21
— 2d exam.,	"	1·10	5·70	43·07

Sesquioxides.

	Amount used in grains.	Water.	Oxygen.	Impure Protoxide.
Braunite,	50	3·27	6·37	40·36
— 2d exam.,	"	3·85	6·57	40·10
New Brunswick,	"	4·52	5·09	40·34

TABLE II.

Mean per centage.

Binoxides.

	Water.	Oxygen.	Oxygen from brown oxide.
Geissen,	1·24	17·79	—
Kilpatrick,	2·26	17·19	17·52
Thuringia,	4·45	14·44	—
New Zealand,	6·38	14·02	—
Table Mountain,	2·22	11·39	—

SESQUIOXIDES.

	Water.	Oxygen.
Braunite,	6·91	12·94
New Brunswick,	9·04	10·18

TABLE III.

Mean of all Experiments.

BINOXIDES.

	Oxygen.	Chlorine.	Binoxide.
Geissen,	17·79	78·93	97·84
Kilpatrick,	17·19	76·27	94·54
Thuringia,	14·44	64·07	79·42
New Zealand,	14·02	62·20	77·11
Table Mountain,	11·39	50·53	62·64

SESQUIOXIDES.

	Oxygen.	Chlorine.	Equivalent to Binoxide.
Braunite,	12·94	57·32	71·17
New Brunswick,	10·18	45·16	55·99

The column headed Chlorine gives the relative value of the oxides for the preparation of bleaching powder, and is obtained by multiplying the oxygen column by 4·437, the atomic weight of chlorine.

2d April, 1851.—*The* VICE-PRESIDENT *in the Chair.*

Messrs. David Tainsh and Paul Cameron, were admitted members.

The following papers were read:—

"Notice of Liebig's new method of Analysing Common Air." By Mr. R. M. Murray.

"Notice of a pure form of Sesquioxide of Chromium and on Sesquichromate of Potash." By Dr. R. D. Thomson.

XXIII.—*The Effects on Health of inhaling the Fumes of Cyanide of Potassium Solutions. By* JAMES NAPIER, ESQ.

I BELIEVE it is a fact well known in medical practice, that different trades and occupations give rise to distinct kinds or forms of disease. I was told a few years ago by an eminent physician, that so much does the modification of disease depend upon the influence of our occupation, that in some trades, such as that of a printer, a change of colour continued in for a length of time will give a distinct form of disease; and that much difficulty is experienced in the medical profession by the paucity of observations upon the causes which influence these modifications. It may

therefore be useful as well as interesting to give a brief notice of the effects produced upon the health of those employed in a new art or trade, namely, electro-plating and gilding; it may be remarked, however, that the effects to be described were under conditions where proper caution was not taken, so that the magnitude of the evils to be referred to is not necessarily connected with the new art, where care is taken, which is the more gratifying, so far as gilding is concerned, as it supersedes a process notoriously injurious to health, even under every care, namely, the gilding by mercury.

The operations of electro-plating and gilding are performed, as is well known, by depositing the metal from its solution by means of electricity. The solutions used are a double salt of cyanide of potassium, and the metal, whether silver or gold, having a great excess of cyanide of potassium, which is constantly undergoing slow decomposition, and giving off fumes of hydro-cyanic acid. The room in which the subsequent observations were made, measured 20 feet by 16 feet, having very imperfect means of ventilation. The vats containing the solution were at one end, exposed a surface of solution of 16 square feet, and contained dissolved not less than from 50 to 70 lbs. weight of cyanide of potassium, so that although the decomposition was slow, the quantity of solution made it great in that size of room, which always smelt strongly of cyanogen gas. In the mornings especially, after being closed during the night, the smell was heavy and sickening.

On entering the room, the first impression was a heavy smell, and after a short time a saline taste was perceptible, producing a dry frothy spittle. This, with other effects, makes me believe that the poison, although in a gaseous state, was not taken into the lungs, but was dissolved by the saliva of the mouth, and went into the stomach; so that the effects are those of taking very minute and constant doses of prussic acid, extending over a period of at least from two to three years.

I may here mention an occasional effect of an intermitting sort. The nose becomes dry and itchy, and breaks out internally into small pimples, so painful that they can hardly be touched. This effect, however, I have often, and more constantly, experienced in breathing the hydrogen evolved from the galvanic batteries, than from the fumes of cyanide solutions, although cyanide fumes produce the same results.

The first permanent impression is languor—a feeling of weariness comes over the body, with an inclination to seek warmth, without feeling any actual sensation of cold. Then follows the want of inclination to eat—the meals being begun as a matter of duty, although when commenced the relish seems restored, and the persons are capable of taking the usual quantity of food. These feelings were worse towards evening than morning, and often long after I was sensible of the effects produced did I feel quite well in the morning. Then the colour of the face becomes pale or sallow, the visage grows sharp, eyes sunk in the head, and a dark colour immediately round them.

As time went on these effects became more permanent. Stomach almost always acid, with flatulence; mouth ill-tasted, and in the mornings dry and parched, and bad breath; occasionally there occurred a feeling of irritability in temper and great restlessness, starting often through sleep, and awaking in fearful dreams; there was then often felt a benumbing sensation in the head, the mind having no individuality, with heavy pains, not acute, shooting along the brow, giddiness, and momentary heavings, as if the earth was making a lurch like a vessel, with occasional gloomy apprehensions of death. The feeling of languor increased, bringing on drowsiness, which, towards evening, was often irresistible, so that I would often fall asleep a few minutes after sitting down anywhere. For several weeks almost every other morning the nose bled copiously shortly after getting out of bed. To all these effects were at last added rushing of blood to the head with a hissing noise; in the day momentary blindness and rapid giddiness, which often lasted several minutes.

With other two individuals this rushing of blood to the head was followed with severe pain and flushing of the face, but in my own case I experienced no flushes of the face nor pain in the head, but a burning sensation and giddiness, and a dimness in the sight which lasted several hours.

I cannot help remarking here, and no doubt anticipating your remarks, how foolish it now appears for us to have remained under such evidently dangerous conditions; I cannot account for it in any other way than from the gradual accession of the symptoms; for although I have described them in a few minutes, they took upwards of two years and a-half of being fully developed; and were so insidious, that although sensible of the cause, it did not strike us as serious until the sudden rushing of blood to the head and blindness sounded the alarm. Our culpability seems more now, knowing how easy it is to avoid the danger by proper ventilation.

In the early stage of the business, when anything went wrong with the solution, the only means of recovering the silver was by precipitating it by an acid, which produces an enormous evolution of gas; for these operations a number of ordinary labourers were employed to clean out the vats. The floor of the room was all sprinkled with ammonia, to neutralise the effects of the prussic acid as much as possible. Some of those men became giddy and had to be led out of the room, not being able to stand it any length of time; others, after some time, became sick and vomited, but after a supply of victuals and fresh air could resume their labour; while not a few stood it without feeling much annoyance beyond what a glass of spirits put to rights. Those whose occupation and habits were of an intellectual sort, were more easily and powerfully affected than those whose occupation and habits were merely physical.

One modification is singular, and has struck me as curious. An individual of a strong healthy constitution, and who felt the effects described upon myself very slightly, although he was longer under its influence; but after breathing it for a few days felt stronger than usual, had severe

ulcerations on his legs and other parts of his body. The bad gas, as it is termed, was blamed, but for some time I could not convince myself that that was the cause; an experience, however, of at least five years, has now completely verified the observation, that these outbreaks are the results of breathing these prussic acid fumes Any circumstance which necessitates the inhaling much of these fumes for a few days successively, gives rise to ulceration, and often the person has to retire for a few days to the coast, where they heal rapidly. Another curious circumstance is, that a person who has wrought in the fumes these last seven years, has completely lost the sense of smell for every thing but strong ammonia; even sulphuret of carbon, using his own language, "smells as sweet to me as a rose."

I have not heard of any fatal results, either directly or indirectly, arising from following this occupation; and in some works where care is taken, the effects I have been describing are produced only in a very mild form, and are only occasionally felt, being considered quite evanescent. One remarkable difference I may notice between the effects of breathing these fumes, and that of inhaling the vapour of quicksilver; in the old process of gilding, pure mercury enters and gets a footing in the system; it undermines it, and permanently destroys health and shortens life; but in the cases I have given of the effects of breathing cyanide fumes, notwithstanding the great want of care in the conditions, and the dangerous nature of the effects, by an absence of a few months from the business the health of all the parties described has been completely restored.

The solutions of gold and silver in cyanide of potassium are deadly poisons if taken into the stomach, but from their extremely disagreeable bitter taste, the slightest drop getting into the mouth is immediately washed out with water, to get rid of the taste,—a great preventative of evil from that source, as small drops often unavoidably get into the mouth, and would be unconsciously swallowed were it not for the taste.

When any part of the skin is broken and comes into contact with the solution, severe ulcerations and great pain are caused. The solution coming upon the healthy skin creates little ulcerations. The operator is much more liable to this when gilding than plating; the gold solution being wrought hot, when it comes in contact with the hands is more powerful. The hands are also much exposed to the steam of the solution, and often, from this cause, break out in watery pimples, which are very itchy and painful. When ulcerations are caused by the solutions, or when the solutions come into contact with ulcerations, the metal is reduced to the metallic state in the sore, and if looked at through a common lens, the metal is quite visible. The reduction and presence of the metal are, no doubt, the cause of the irritation and pain.

I have thus briefly endeavoured to give the results of observations upon the effects experienced by myself, and at least four other individuals, during a period of several years' working in prussic acid fumes.

And although these observations are in plain language, I have no doubt but the medical members present will be able to apply them in a proper way. They will also be a warning to young experimenters who use cyanide solutions in their electrotype operations, and who occasionally keep these in presses in their bedrooms, such practices being very injurious.

I may also mention another thing in connection with this and other operations where deleterious gases are evolved, that clothes absorb great quantities of gas; and the individual who works in these gaseous fumes and does not change his clothes when he leaves the work, will move about like a distinct planet, having an atmosphere of his own in which he lives and breathes for many hours after leaving his employment. This fact cannot be sufficiently impressed upon the mind of both workmen and employers, who should urge the proper conditions of health—a subject I have some hopes of bringing before the Society on a future occasion, in a more formal manner.

16*th April*, 1851.—*The* Vice-President *in the Chair.*

Dr. Allen Thomson continued his observations "On the Relations of the Nervous and Muscular parts in Animals," and intimated his intention of resuming the subject next session.

Dr. Andrew Buchanan made some remarks on the importance of the inquiries prosecuted by Professor Thomson, more especially in their bearings upon the philosophy of the mind.

April 30, 1851.—*The* Vice-President *in the Chair.*

The concluding meeting of the Session was held this evening.

Mr. J. G. Houston was admitted a member.

Mr. William Ferguson read "Notes on the Geology of Dunbar Shore," illustrated by maps, drawings, sections, and fossils.

Mr. J. P. Fraser read a paper by Mr. John Gray, "On the Lepidoptera of the Vale and Frith of Clyde," illustrated by specimens.

A paper by Mr. George W. Brown was read, "On the Composition of Orkney Kelp."

Mr. Keddie gave in the following Report from the Botanical Section:—

Botanical Section.—April 22, 1851.—Dr. Walker Arnott *in the Chair.*

Dr. Thomas Thomson, jun. read the following paper, illustrated by specimens presented by him, through Dr. Robert D. Thomson, to the Herbarium belonging to the members of the Section.

XXIV. — *Sketch of the Climate and Vegetation of the Himalaya.* By THOMAS THOMSON, M.D., Assistant Surgeon in the H.E.I.C. Service, Bengal Establishment.

THE great range of the Himalaya, when taken in conjunction with the still more elevated mountains behind, which are in nowise distinguishable from it, constitutes the most stupendous mass of mountains in the world, not only from containing the highest peaks, but also, and still more remarkably, as presenting by far the greatest area of elevated land.

This gigantic mountain mass lies to the north of the great plain of India, from which it rises on the whole very abruptly. It has a direction very nearly from east to west, its west extremity is however a little more northerly than the east, the latitude rising from 26° at the east, to 33° at the west extremity.

The mountain chain to which the name of Himalaya is most properly applied, may be considered as bounded at the south by the plains of India, and on the north by the rivers Indus and Burrampooter, which have their sources in the same spot, and run one to the east, the other to the west, among lofty mountains, till they enter the Indian flat country. Nearly in the centre of this chain, in the most westerly part of Nepal proper, lies the point of separation between the two river great systems, that of the Indus and that of the Burrampooter, constituting a north and south axis, which, when better known, will probably prove to be the grand axis of Asia.

From this centre the chain of the Himalaya extends to nearly an equal distance in both directions, the central axis of the chain being the line of water-shed between the streams which run toward the plains of India on the south, and those which flow toward the Burrampooter and Indus on the north. This line of water-shed or central axis will, on inspection of a map, be seen to be in general somewhat to the north of half way between the two boundary lines of the chain, so that the distance from the axis to the plains of India, is greater than from the same place to the northern rivers. The mean width of the whole chain may be stated roughly to average about 150 miles, of which 90 are to the south of the line of water-shed and 60 to the north of it.

From the central axis of the chain, lateral ranges of mountains run both to the north and south, stretching in the latter direction as far as the plains of India, and separated from one another by deep narrow valleys, which extend far into the interior of the mass of mountains.

The number of lateral chains of the first class which form the line of division or water-shed between the basins of the great rivers on the south side of the central axis of the Himalaya, is about fourteen, separating from one another in a series from left to right the waters of the Jhelum, the Chenab, the Beas, the Ravi, the Sutlej, the Jumna, the Ganges, the Gogra, the Gandak, the Kosi, the Teesta, and the Subhansheri. These great chains, like the central axis, throw off lateral branches,

which separate from one another the different branches, by the union of which within the mountains the great rivers are formed.

The elevation of the central axis of the Himalaya is probably at a mean about 18,000 or 20,000 feet; it is nearly uniform at about these elevations throughout a great part of the chain, but gradually diminishes toward both ends. Like all mountain chains, it presents alternations of high and low portions, the lower parts or passes as they are called, from their affording the means of passage to travellers from one side to the other, being at the upper extremities of the river basins. These passes are, with a few exceptions, rarely under 17,000 or 18,000 feet. The lateral chains, starting from the more elevated portions of the central axis between the passes, gradually diminish in elevation as they approach the plains of India, not however with any exact uniformity of progression, for it is not unfrequent to find them rise into lofty peaks considerably more elevated than any known part of the central axis. The greater part of the giant peaks, which rise to an elevation of 26,000 or 28,000 feet, are situated in this manner, not on the central axis, but to the south of it; it is however by no means improbable that masses of equal elevation not yet measured or observed may occur behind them, it being unquestionable that the general elevation of the country continues to increase as we advance to the north, and that we have not yet (except in one place) attained to any point from which a descent is commenced towards the northern plains.

The direction of the principal lateral chains and of their included valleys, is on the whole perpendicular to the main axis, but with an inclination from the centre; those on the extreme east inclining to the eastward, while those on the extreme west have a very westerly direction. There are certain anomalies in the courses of the rivers, particularly at the north-west extremity of the chain; which, however, may be overlooked in a view so general and cursory of these rivers, as must necessarily be taken on the present occasion. The most marked of these peculiarities may be observed in the course of the Sutlej, which runs for a very considerable part of its course nearly parallel to the Indus before it turns toward the plains, thus separating the western part of the Himalayan chain almost from its very origin into two branches, one of which separates the Sutlej from the Indus; the other to the south of, and nearly parallel to the other, divides the basin of the Sutlej from that of the Jumna and Ganges.

From the great depth of the valleys which separate the different mountain chains, it but seldom happens that any road crosses from one valley to another, a traveller has therefore, in general, excellent opportunities of studying the direction and ramifications of the different chains, either in following the course of the valleys, or by travelling along the top of the ridges. In both cases he will find that his course is an undulating one, each chain and each branch of a chain being a curve, which bends first to one side and afterwards to the other, giving off generally a spur on the convex side, while the head of a valley insinuates itself into the concavity.

After these few words on the physical structure of the mountains, the vegetation of which it is my wish briefly to describe, it will still be necessary to devote a few minutes to the subject of climate and humidity, before I can proceed to my proper subject.

Situated in the most southern part of the temperate zone, and bounding on the north a great peninsula, which extends far into the torrid zone, the base of the Himalaya to the south possesses an almost tropical climate, tempered however when the sun is on the tropic of Capricorn by a moderately cool winter, and variously modified in different parts of the chain by the degree of humidity, a most important matter to be taken into consideration in every question connected with the phenomena of vegetable life.

The source of humidity in the Himalaya is almost entirely the Bay of Bengal, which is situated about 5 degrees to the south of the eastern extremity of the chain; and the wind which carries the humid atmosphere along the chain, is that which is known to nautical meteorologists as the south-west monsoon, a wind which begins to blow in the open sea about the month of April, but whose effects are not felt in the far interior before the month of June. This wind, though constant in its direction at sea, is not so in its inland course; at the head of the Bay of Bengal it is almost a south wind; it blows from the sea nearly due north towards the Himalaya, striking in its course upon the low chain of the Khasya hills, whose maximum elevation is scarcely 7,000 feet.

Upon this range the first force of the monsoon is expended, and the annual fall of rain at Churra Poonjee, elevated 4,000 feet on its southern slope, amounts to about 500 inches. This range, which has its origin among the mountain ranges of the south of China and north of Burmah, lies to the south of the Burrampooter, and following the course of that river, terminates in the concavity of its great bend, where it turns down toward the sea. The Khasya mountains do not therefore entirely run across the Bay of Bengal, so as to intercept the force of the monsoon from the whole of the Himalaya, a part of which wind, laden to saturation with moisture at a temperature of nearly 90° F., blows due north from the Bay of Bengal upon the district of Sikkim, which is on that account the most rainy part of the whole range of the Himalaya, for, on the one hand, the more eastern parts of the chain are protected by the Khasya range, and on the other, the more westerly parts are more distant from the source of moisture, and therefore receive a less share of it. The interception of the moisture from the province of Bootan and the independent states north of Assam, by the Khasya range, has this curious effect, that the lower ranges of this portion of the Himalaya are dry and arid, while above 7,000 feet, to which elevation only the hills to the south attain, the climate is very much more humid.

The diminution in the amount of moisture in proceeding to the westward along the Himalaya from Sikkim is extremely gradual, but also so far as our at present rather limited number of observations goes, very

regular. The effects of the south-west or rainy monsoon diminish step by step, as we advance westward, till on arriving at the valley of the Indus at the western extremity of the Himalaya, it ceases to be observed at all. In these most western portions of the chain, very little rain falls at any season of the year, and the little which does occur, falls in the spring months, and is therefore quite independent of the regular monsoon.

It is also worthy of note, that in the more western parts of the chain, the climate is extremely dry at all periods of the year, except during the monsoon or rainy season, as it is called in India, while to the eastward the climate of the mountains shares to a considerable extent the more equable and always moist climate of Bengal.

The most important point of all, however, regarding the climate in respect of its effects on vegetation which requires to be borne in mind, is that a very great portion of the rain which falls is deposited on the first range of mountains upon which the rain wind strikes. I have already pointed out that this is the case with the Khasya range, and it is there highly strikingly illustrated by the fact, that it is only on the very south side of the hills that the rain fall is so enormous, the fall twenty miles north of Churra being probably less than half what it is there.

This tendency of the rain fall to exhaust itself very considerably on the first range of mountains to which it has access, is peculiarly important in a mountain chain 150 miles in width, its effect being that the upper part of all the large valleys, and especially the interior valleys and their ramifications, are much more dry than those adjacent to the plains of India. Even in the most humid part of the Himalaya, in Sikkim, this difference is extremely marked, and in the more dry parts to the west, (the extreme east interior is not known,) the inner valleys are so dry that rain is scarcely ever known to fall.

In close connection with the increase of aridity, as we advance from the plains of India to the interior of the mountains, I may mention the increased elevation of the line of perpetual congelation, which has evidently the same cause. In the outer lofty ranges of the Himalaya, the snow line is met with at about 16,000 feet, while in the Tibetan part of the chain, many ridges of 20,000 feet of elevation are almost entirely bare of snow.

Having thus alluded in very brief and general terms to the most prominent physical features of the mountain chain of Himalaya, I shall proceed to describe, as rapidly as is consistent with clearness, the general character of the vegetation which is to be observed in its different parts at all elevations, from the plains of India to the uppermost limit of vegetable life. This would be an easy task if the vegetation were uniform throughout the whole chain, but owing to the great variations of climate to which I have just adverted, there is a very great difference in this respect, few indeed of the plants of the eastern extremity of the Himalaya being identical with those which occur in the far west. In general terms, it

may be said, that to the eastward the vegetation is very much more luxuriant and tropical, and that it changes very gradually in advancing to the westward, in exact proportion to the diminution in the quantity of rain. The same gradual transition in the vegetable world may also be observed in advancing up the valleys, or in passing across the mountains from the outer valleys to those which are further removed from the Indian plain; though in the latter case, of course, the effects of gradually increasing elevation must be taken into consideration as partly the cause of the change as well as the decrease of humidity.

The plains of northern India which skirt the base of the Himalaya, do not (if we except the belt immediately at the base of the mountains,) present by any means a rich flora. From their situation nearly on the tropics, their distance as a whole from the sea, and their proximity to the mountains, they are not very damp, and their climate has too decided a lowering of temperature in the cold season to permit them to be clothed with the dense forest vegetation which clothes the tropical plains of South America. They are in general open plains without much wood, and where not under cultivation, are covered either with a dense jungle of different species of arundo and saccharum, or with scattered trees of various tropical families, acaciæ and zizyphi being very common genera. Here and there only there are patches of forest generally low and scrubby, and without much underwood, or any of the fine parasitical plants and ferns which are so ornamental in tropical woods.

In the lower parts of Bengal, the proximity of the sea somewhat modifies this general character; a number of ferns, one or two species of pothos, and a few Orchideæ, among which Vanda Roxburghii and a large and fine Cymbidium are the most common, are to be found. In the same way the valleys of Silhet and Assam are exceptional in character, but from their being inclosed with mountains of some elevation on all sides, they are scarcely to be regarded as part of the Indian plain, but may more properly be considered as wide mountain valleys, and they in fact closely resemble in vegetation the valleys of the larger Himalayan rivers in the east part of the chain.

Close to the foot of the chain of mountains throughout its whole course from east to west, there lies a belt of forest and swampy land, which is well known in India by the name of Terai, and which, where it is developed to any considerable extent, bears a very bad character for unhealthiness, and is indeed in many places quite impassable for Europeans at most seasons of the year. This forest belt seems to be due to the greater humidity of atmosphere, and at the same time greater equability of temperature, which is produced by the proximity of the mountains. Its width is very various, from forty or fifty miles, to which I believe it attains in some parts of Nepal, to eight or ten miles, which is a more common width. Westward of the Jumna it almost disappears, being represented by a line of swampy or marshy ground, and a low jungle of bushes of the common plain species of trees.

In this belt, which occupies the base of the mountains, the vegetation is of course quite tropical in character, and is too varied to be described in detail. Large cotton trees (Bombax) are in all parts of it particularly conspicuous from the immense size of their trunks, which are not cylindrical, but buttressed all round by immense plates which project far forward from the main trunk. Numerous fig trees of very various species are also common, especially to the eastward, where many fine forms of these magnificent trees everywhere meet the eye, along with species of Dillenia, Careya, Bauhinia, and Lagerströmia.

It is from the forest which lies along the foot of the Himalaya that a great part of the timber is derived which is consumed in northern India. In the most eastern part, the most valuable timber is furnished by Lagerströmia reginæ, and perhaps other allied species; further west, the *sâl* Patica robusta, the Shorea robusta of Roxburgh, is that which is most esteemed. The sâl extends from the valley of Assam as far west I believe as the Punjab, and is found not only in the forest tract, but also in hot valleys among the mountains. It belongs to a natural order (dipterocarpeæ) which is peculiarly Indian, and which furnishes many valuable kinds of timber. None of the species, however, except the one under consideration, extend beyond the tropics; but they abound in the hilly countries of the peninsula as well as in the low ranges of the Malayan peninsula, and I believe in Java and other Indian islands. The sâl is so much valued that it has become in accessible places from whence it can easily be conveyed to the plains, very scarce, and in the vicinity of large towns where there is a great demand for timber, I believe almost extinct. It is therefore less commonly employed than the sissoo, a species of Dalbergia, which is particularly abundant along the foot of the mountains, more especially to the westward, growing in great profusion on gravelly soil, and yielding a most ornamental and valuable wood.

The forest belt which skirts the base of the mountains rests for the most part upon a dry gravelly soil, which slopes somewhat rapidly, though not perceptibly to the eye, toward the open plains, and is generally dry. Just outside the forest, or sometimes still interspersed with patches of wooded ground, there is generally a low swampy tract, which is lower than the country immediately beyond, and from which the water drains away slowly and with difficulty. This is the Terai *par excellence*, and is, from the constant dampness of the soil, and the dense heat of the summer, peculiarly unhealthy. It is too low and too unhealthy to be much cultivated, and is generally covered by a dense jungle of tall grasses, species principally of Saccharum, Arundo, Andropogon and Anthistiria, which rise high enough to cover an elephant, and afford shelter during the greater part of the year for multitudes of tigers and other wild animals; at the commencement of the cold weather, this long grass is set on fire and burnt down by the inhabitants of the hills, who at that season descend to the level country to feed their cattle and flocks. It is again abandoned to itself at the commencement of the hot season, as soon as grassy vegeta-

tion has made sufficient progress in the mountains. These swampy tracts are a series of lateral valleys which run parallel to the base of the mountains, and which, from being very slightly inclined, present great obstacles to the escape of the water discharged into them by numerous streams from the mountains.

Along many parts of the Himalaya, a similar series of valleys nearly parallel to the axis of the chain, but bounded externally by hills of from 2,000 to 4,000 feet in elevation, may be observed. These valleys are known in the western Himalaya by the name of Dhúns. One of the largest of them is the Deyra Dhún, well known to Indian travellers as being traversed en route to Masuri a favourite hill station, and now celebrated as the seat of an extensive cultivation of tea in a climate which seems to suit admirably that valuable plant. The Deyra Dhún is in its centre or highest part, from which it slopes down both to east and west towards the Ganges and Jumna, about 2,500 feet above the level of the sea, or 1,500 feet above the level of the plains, immediately outside of its bounding range.

Other Dhúns occur all along the hills to the westward. They are bounded on the north by the ancient rocks of the Himalaya, but on their outer side always by the tertiary sandstones and conglomerates, now so well known from the labours of Falconer and Cautley, as the Sewalik formation. In the north of the Punjab there are often several series of these valleys, the innermost only resting on transition rocks, the others excavated out of the tertiary sandstones, which have there often a width of from 30 to 50 miles.

The vegetation of the low ranges of hills by which the Dhúns are bounded externally, does not deviate much, if at all, from the tropical type. They nowhere exceed an elevation of 4,000 feet, which is not sufficient in isolated ridges, to bring about a sufficient change of mean temperature, to produce much alteration in the vegetation. They are only known, I believe, to the westward of Nepal, and therefore, in the drier parts of the region, they are generally covered with trees the same as those of the forest belt, with, in addition, a good deal of pinus longifolia, a subtropical species of pine, and of a dwarf species of Phœnix, almost the only palm of the western Himalaya.

From these valleys where they exist, or from the open plains in other cases, the exterior ranges of the Himalaya generally rise abruptly to a height of 7,000 or 8,000 feet, in all parts of the chain, except at the point of exit of the great rivers, where of course the outline of the mountains is much modified. I shall probably better explain the structure of the mass of mountains, by saying that the lateral chain which separates any two adjacent river basins, generally terminates abruptly towards the plains in a bold promontory 7,000 or 8,000 feet in height, from which lateral branches parallel to the plains run in each direction, gradually diminishing in elevation till they are terminated by the great rivers. After the first sudden rise, the different ridges increase much more gra-

dually, generally running nearly level for a number of miles, and then rising abruptly from 1,000 to 2,000 feet.

In ascending on the Himalaya (or indeed on any range of mountains) from the base to the line of perpetual snow, the change of vegetation is extremely gradual, and within a limited change of altitude barely perceptible, any division into groups must therefore be in a great measure arbitrary. Still some mode of subdivision is quite necessary for the purpose of description, as otherwise the mind would be puzzled by the multitude of facts. The less complicated, however, the mode of division is, the more intelligible it will be; it appears therefore quite sufficient to refer the forms of vegetation to three groups, similar to the three zones interposed between the equator and the pole, namely, tropical, temperate, and arctic; or to use the term more commonly applied in the case of mountains, *alpine* vegetation.

There is so great a diversity in the vegetation of different parts of the Himalaya, that I should entirely fail, were I to attempt to give any general idea of the vegetation of these different zones. I shall therefore select two particular spots, and by relating in some detail the gradual changes of the vegetation in each of these, I shall, I hope, be able to give a good general idea of the general appearance of the phenomena of vegetable life.

The hill station of Darjiling is distant from the plains of Bengal a little more than 36 miles, the road following a ridge which ascends in the first 13 miles rapidly to about 7,000 feet, and then runs gradually with little change of level for the remainder of the way. Throughout the whole distance the mountain sides are lined with dense forests; except in the early morning, an almost perpetual mist hangs over the trees, which collect and throw down from their foliage an abundant supply of moisture. On emerging from the dry belt of tropical forest, the ascent commences at once up a dry ridge, covered at first with the same species as grow upon the plain, species of Bombax, Terminalia; Sterculia, Emblica Duabanga, Alstonia, Gmelina, Bauhinia and others are abundant, with many figs, some species of Artocarpus, and a proportion of bamboos. By degrees a vegetation characteristic of mountain tracts, but still tropical, takes the place of those just mentioned. A Gordonia is extremely abundant, with numerous euphorbiaceous trees allied to Mappa, various species of Garcinia, the toon, (Cedrela toona or serrata,) a variety of mimoseous trees, arboreous species of Vernonia and Helicia, beautiful Bauhiniæ both erect and scandent, the latter climbing to the tops of the highest trees with a trunk nearly as thick as a man. The road runs along the top, or on one side of the ridge, looking down into deep valleys full of the densest forest. If we leave the road to enter into these dark and moist hollows, we find that there are occasionally small tracts of flat land along the banks of the streams, which, however, more frequently run through deep ravines, clothed with dense thickets of shade-loving trees, species of laurel, alder, magnolia, being mixed with the giant figs, which often form a great part of the

forest. In these more shady places the plantain and tree fern luxuriate, and a dense brushwood covers the ground. Not unfrequently large tracts are covered with thickets of Calamus, a prickly palm which attaches itself by long hooked flagelli to the trees, and often presents a formidable barrier to the traveller who tries to penetrate into its recesses. The trunks of the trees are often clothed with a dense mass of Pothos, and of the huge leaved Scindapsus, completely encircling them all round, and converting them into leafy columns, while the wide-spreading branches of the higher trees bear a profusion of Orchideæ, which overspread them even to the very top, and, when in flower, have a most gorgeous effect.

In shady valleys, as low as 2,000 feet, appear the first specimens of oaks and chestnuts, which in the equable temperature of such places, descend much further on the mountain slopes than in the more arid and variable climate of the western Himalaya.

On attaining an elevation of about 6,000 feet, the vegetation has become temperate. The purely tropical forms have almost entirely disappeared, and in their place the forest abounds in trees of temperate climes. Species of oak, holly, cherry, laurel, Rhododendron, Styrax, and Magnolia, of gigantic size, form the forest, densely covered with mosses and orchideæ, and with an underwood of species of Berberis, Daphne, Lonicera, many species of Vitis, and smaller species of bamboo than those of the tropical region. Ferns are at such elevations extremely abundant.

From the station of Darjiling, the view in every direction overlooks mountain ranges, covered with dense forest, except in a few spots where partial clearances have been made for cultivation. No bare or grassy mountains meet the eye, no rocks or precipices afford any relief from the prevailing uniformity, which, but for the magnificence of the snowy mountains behind, would be undoubtedly monotonous and fatiguing.

The ascent from the plains of north-west India to Simla, is about the same length as that to Darjiling, but presents the most marked contrast in vegetation, being throughout bare and grassy. The road ascends at first in ten miles to an elevation of 6,500 feet, then descends to about 1,000 feet, and ascends gradually to 5,000. The ascent commences from the Pinjore Dhún, a lateral valley which runs at the foot of the mountains from the Sutlej to the Jumna rivers. There is no forest in this valley, which is open, and to a great extent cultivated. The lower hills are covered with a shrubby vegetation characteristic of a dry climate. Species of Zizyphus, Carissa, Butea, Adhatoda, Bergera, Ægle, Flacourtia, and other common shrubs, with one species of bamboo, and only one fig. After the ascent commences, these bushes are only scattered at intervals over the hills, the greater part of the surface being bare and grassy. A similar open country extends all the way to Simla, except where a few fir trees (Pinus longifolia) crest the ridges, and in the more shady ravines, which are lined with a few small trees.

The transition from tropical to temperate vegetation begins, in so far as it is indicated by the small amount of shrubby vegetation, at about

5,000 feet, but on the more exposed slopes, plants of warm climates extend up 1,000 feet higher, and the herbaceous vegetation, principally grasses, is entirely composed of tropical forms.

It is only on approaching Simla, and attaining a height of nearly 7,000 feet, that forest vegetation commences; at that elevation, open forests of oak, Rhododendron, and Andromeda, intermixed with several species of pines, and a great number of temperate shrubs, of such genera as Rosa, Rubus, Viburnum, Berberis, Spiraea, Lonicera, Indigofera, Prinsepia, Salix, Daphne, and others.

The view from Simla presents a very marked contrast with that from Darjiling. The general outline of the mountains is very much the same, but they are more rocky, and very generally bare; the forests, which to the north are dense and abundant, occupying chiefly the north slopes of the mountains, so that in looking from the south the crest of the ridges only are seen to be wooded. The scenery, therefore, is more diversified than in the eastern Himalaya, and abstracting the snowy mountains, more pleasing to the eye.

Between the two extremes which I have described, every intermediate form may of course be met with, the law of alteration being apparently the following, that in advancing westward towards less humid climates, the lower hills from about 6,000 to 2,000 feet, become more and more bare and grassy, while the lower levels and the base of the mountains retain a greater degree of damp and are clothed with forest. It would appear also that above 6,000 or 7,000 feet, up to 10,000 or 11,000 feet, at which elevation mountain ranges sensibly interrupt the passage of the moist atmosphere, the temperate ranges are more moist than those below them, which do not collect the clouds, and have a higher temperature, and consequently more powerful sun. To the eastward of Sikkim, the same phenomena are very well marked, the lower ranges being extremely dry and arid, while above 7,000 feet, dense forest and a humid atmosphere prevail, just as in the mountains of Sikkim.

The valleys of the larger rivers which traverse the Himalaya from north to south, have of course a much lower elevation than the mountains by which they are surrounded; and up them, therefore, tropical vegetation penetrates very far into the interior. In the extreme west, the valleys of the Indus and Chenab, and even of the Sutlej, are up to the height of 5,000 feet, which they do not attain till more than 100 miles from their exit into the plains, hot, dry, and tropical. Further east, the tropical forest stretches far up the vallies, and they are only bare for a small portion of their extent, and in the humid atmosphere of Sikkim they are densely wooded throughout. In that province, the valleys of the Teesta and its tributaries, carry tropical vegetation far into the interior, almost within a day's journey of the line of perpetual snow, and the luxuriance of the dense and dripping forest requires to be seen to be understood.

The temperate region of the Himalaya may be said to extend from about 5,000 feet, or a little above it, to the upper limit of arboreous vege-

tation; which, to the westward, is about 12,000 feet, to the east about 1,000 feet higher. Above 9,000 feet, however, the temperate region is characterized by many remarkable forms, which do not extend lower; these are generally, in the west especially, of very European type; but in the eastern flora, it is at such levels that the magnificent Rhododendrons of Sikkim, which form so striking a part of its flora, principally occur. In this zone a great part of the trees are of European genera, alders, oaks, birch, hazel, hornbeam, horse-chestnut, and cherry, being characteristic forms. It is also especially the region of coniferous trees, very few of which, extend either below or above it. The pine which descends to the lowest level in the Himalaya, is Pinus longifolia, which is a common tree throughout the whole region from the mountains of the Punjab to the east of Bootan. It is confined in a great measure to the outer ranges of the mountains, and commences as low as 1000 feet above the level of the sea, rarely if ever attaining a greater elevation than 7,000 feet. This tree appears to have a very great power of enduring varieties of climate, for it seems equally at home in the hot, damp valleys of Sikkim, surrounded by an entirely tropical vegetation; and on the dry stony hills of the Punjab where rain hardly ever falls, and it is at all seasons exposed to a powerful and scorching sun. The only other coniferous tree of low elevations in the Himalaya is Podocarpus, one species of which is a native of the lower ranges of Nepal and Sikkim.

Pinus excelsa, which is allied to P. strobus, and Pinus Smithiana, which is near abies, are the more common species of the central zone, which are distributed throughout the whole extent of the Himalaya. In the same zone, the deodar (Cedrus deodara) is confined to the western mountains, not being, I believe, to be found indigenous in any part of Nepal, while P. Brunoniana, on the other hand, commences in the eastern parts of Kamaon, and extends as far east as Bootan. The most alpine species of the family are P. Gerardiana, P. Webbiana, and several species of juniper, of which all but the first, which is a western tree, seem universally distributed.

It would be needless to dwell at any length on the alpine zone, because luxuriant as is the vegetation, and beautiful as are the plants, the forms at least must be familiar to most of my auditors.

I must be content, in conclusion, with drawing your attention to the change produced in the vegetation in the temperate and subalpine zones as we advance towards the interior of the mountains, in consequence of the diminution in the amount of rain.

If in travelling through the Himalaya we ascend a great river, the ascent is so very gradual, that the change of climate and of vegetation in ascending is almost imperceptible, and is only detected by careful observation. If, however, on the other hand, we cross a range of considerable elevation, and descend on its northern side into another valley, the transition is often very striking, and if the chain be sufficiently elevated to intercept the greater part of the rain, the contrast between its two sides

is perfectly astonishing; when the transition is thus complete, the traveller leaves dense forests and common Himalayan vegetation on the one side, to find on the other a dry barren burnt up soil, with scattered Astragali, Boragineæ, and Cruciferæ, of forms quite characteristic of the flora of Siberia. Such is the vegetation of Tibet, which may be reached either suddenly by crossing a lofty pass, or gradually by ascending the Indus, the Chenab, the Sutlej, the Ganges, and many other of the Himalayan rivers. This arid vegetation is met on the Sutlej as low as 10,000 feet above the level of the sea, and is therefore in no way dependent upon mere altitude.

The members then adjourned to the Hall of the Andersonian Institution, to witness the experiment of the Rotatory Motion of the Earth, which was previously explained by Mr. Crum.

XXV.—*Thermometric Observations for* 1850, *made at Windsor Terrace, Glasgow, by* James King, Esq.

Height of Thermometer above the level of the sea, 94·14 feet.
Taken at 9 o'clock A.M., and 9 o'clock P.M.

JANUARY.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	32·	36·	13,	33·25	29·	25,	39·5	42·
2,	40·	40·5	14,	30·	27·5	26,	34·5	31·
3,	43·75	43·5	15,	25·	28·	27,	25·5	36·
4,	39·	37·	16,	30·5	32·5	28,	43·5	44·
5,	33·5	32·	17,	18·	24·	29,	36·	33·5
6,	25·	25·	18,	30·	32·	30,	34·75	36·
7,	25·	25·	19,	33·5	32·5	31,	36·	44·75
8,	31·	34·5	20,	32·	30·5			
9,	31·	32·	21,	29·	31·	Mean for Month,	33·153	34·169
10,	29·	32·	22,	36·	38·5			
11,	32·5	31·5	23,	41·	42·5			
12,	34·	34·	24,	42·	41·	Mean corrected,	34°·3	

FEBRUARY.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	46·5	45·75	12,	35·	33·5	23,	45·75	45·75
2,	46·5	41·5	13,	30·5	35·	24,	45·	45·75
3,	42·	41·75	14,	42·	48·	25,	44·75	47·
4,	40·	42·	15,	49·5	45·	26,	45·	47·5
5,	37·5	38·	16,	39·5	43·	27,	46·75	47·5
6,	41·	34·	17,	45·	48·5	28,	46·25	45·75
7,	34·	33·5	18,	48·75	49·5			
8,	35·5	42·5	19,	48·5	52·	Mean for Month,	42·65	42·946
9,	45·	38·	20,	45·	43·75			
10,	35·5	37·5	21,	46·25	50·5			
11,	39·	33·5	22,	48·25	46·5	Mean corrected,	43°·6	

MARCH.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	48·5	47·5	13,	44·5	47·5	25,	29·75	34·
2,	48·5	50·5	14,	48·25	47·25	26,	31·	33·
3,	46·5	42·	15,	46·	46·75	27,	32·75	33·
4,	35·75	40·	16,	45·	43·	28,	29·25	33.
5,	42·75	48·	17,	42·5	40·75	29,	36·75	40·
6,	48·5	49·75	18,	40·5	44·75	30,	39·75	40·
7,	50·	48·	19,	46·5	48·	31,	40·5	46·5
8,	48·5	47·	20,	45·5	47·5			
9,	46·75	44·75	21,	42·5	47·	Mean for Month,	41·862	42·475
10,	45·	40·	22,	45·5	44·			
11,	37·75	38·5	23,	38·25	33·5			
12,	41·	41·5	24,	33·5	29·75	Mean corrected,	43°·1	



APRIL.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	48·5	51·	13,	44·75	43·	25,	42·5	42·5
2,	43·5	46·	14,	45·	44·	26,	46·	44·75
3,	51·	49·75	15,	42·75	42·	27,	47·75	45·
4,	51·25	50·5	16,	45·	46·25	28,	46·5	40·
5,	49·	47·	17,	49·75	45·75	29,	42·	44·
6,	41·75	44·	18,	48·75	50·	30,	45·	46·5
7,	50·5	48·75	19,	51·75	50·75			
8,	51·25	49·5	20,	46·5	49·	Mean for Month,	46·6	46·11
9,	49·	49·75	21,	48·75	45·			
10,	46·5	47·	22,	45·5	43·			
11,	44·5	47·	23,	44·	43·	Mean corrected, 46°·4		
12,	45·	42·5	24,	45·	46·25			

MAY.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	45·	45·5	13,	47·	45·25	25,	51·5	51·
2,	46·	43·75	14,	47·75	43·25	26,	53·25	52·5
3,	49·75	43·75	15,	45·	48·5	27,	54·	53·
4,	46·25	41·75	16,	51·	51·5	28,	57·	58·
5,	40·5	38·	17,	56·	52·75	29,	57·	60·
6,	40·75	41·5	18,	52·75	51·	30,	62·5	59·5
7,	41·75	45·75	19,	53·25	54·25	31,	56·5	57·5
8,	43·5	38·75	20,	52·	49·75			
9,	40·	43·25	21,	49·5	54·	Mean for Month,	49·83	49·31
10,	48·	46·25	22,	52·	54·75			
11,	49·75	45·	23,	52·25	56·5			
12,	48·25	45·	24,	55·	57·5	Mean corrected, 49°·7		

JUNE.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	59·	60·25	13,	55·	54·	25,	61·5	60·5
2,	64·5	63·5	14,	53·	48·25	26,	61·5	57·
3,	66·	64·	15,	52·	52·25	27,	55·5	58·5
4,	65·	61·75	16,	52·5	52·5	28,	59·75	55·
5,	60·75	56·25	17,	52·5	52·75	29,	55·	52·75
6,	59·	53·25	18,	56·	57·	30,	53·	56·5
7,	59·5	54·25	19,	59·5	61·5			
8,	58·	54·5	20,	62·	61·	Mean for Month,	58·18	57·06
9,	56·	56·	21,	59·25	57·			
10,	56·75	58·75	22,	57·	58·5			
11,	58·	55·5	23,	60·5	61·75	Mean corrected, 57°·3		
12,	54·5	56·25	24,	63·	61·			

JULY.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	57·	51·75	13,	62·	66·75	25,	61·25	59·
2,	56·75	55·75	14,	63·5	62·5	26,	61·5	60·
3,	56·75	54·5	15,	62·	64·	27,	64·75	64·75
4,	53·5	55·	16,	64·75	61·25	28,	63·5	60·
5,	55·25	55·	17,	63·5	64·5	29,	58·5	63·
6,	52·	53·	18,	62·25	61·	30,	64·5	66·5
7,	56·5	54·5	19,	62·75	63·25	31,	65·75	59·75
8,	55·	53·25	20,	61·25	62·75			
9,	56·75	56·25	21,	65·75	65·5	Mean for Month,	60·53	60·09
10,	56·5	58·75	22,	63·5	63·75			
11,	62·75	61·25	23,	66·	61·5			
12,	62·	64·5	24,	58·75	59·5	Mean corrected, 60°·2		

AUGUST.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	59·5	63·	13,	57·75	58·75	25,	58·75	56·25
2,	62·75	59·75	14,	62·75	61·5	26,	54·5	49·75
3,	61·	60·5	15,	65·	62·5	27,	52·25	46·75
4,	60·75	60·25	16,	62·25	62·5	28,	49·75	49·
5,	60·5	59·75	17,	61·5	57·75	29,	52·75	49·75
6,	58·75	58·5	18,	58·	56·75	30,	50·	52·75
7,	55·	60·75	19,	54·75	51·25	31,	55·25	56·75
8,	62·25	62·	20,	52·5	50·			
9,	59·75	59·5	21,	50·5	47·	Mean for Month,	57·09	55·93
10,	60·5	60·75	22,	52·75	47.			
11,	58·5	56·5	23,	51·5	47·			
12,	59·	57·75	24,	49·	52·	Mean corrected, 56°·9		

SEPTEMBER.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	59·25	57·5	13,	51·75	52·	25,	51·	56·75
2,	58·75	57·25	14,	50·75	50·75	26,	55·	54·5
3,	58·5	50·5	15,	52.25	50·5	27,	55·	55·5
4,	44·5	49·	16,	48·	50·5	28,	53·25	46·75
5,	44·25	50·5	17,	47·	53·25	29,	49·5	45·75
6,	52·	48·75	18,	55·	53·	30,	45·	47·
7,	50·	46·25	19,	54·75	56·			
8,	47·25	49·75	20,	54·25	55·25	Mean for Month,	51·74	51·71
9,	45·75	49·5	21,	55·	52·25			
10,	48·5	52·	22,	54·75	54·			
11,	54·	52·75	23,	54·5	53·25	Mean corrected,		52°·4
12,	52·25	52·75	24,	50·5	48.			

OCTOBER.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	52·	46·75	13,	49·5	49·75	25,	41·5	40·5
2,	41·5	47·75	14,	51·25	39·5	26,	34·	39·5
3,	49·5	47·	15,	40·5	47·	27,	40·	42·75
4,	51·	46·	16,	50·75	51·	28,	38·25	37·
5,	44·75	44·5	17,	51·5	52·25	29,	37·	39·25
6,	47·	50·	18,	53·	54·75	30,	47·75	48·5
7,	49·25	50·	19,	52·25	48·25	31,	48·	50·
8,	50·75	55·75	20,	45·5	43·75			
9,	46·75	43·	21,	43·5	39·75	Mean for Month,	45·096	45·516
10,	44·	44·	22,	35·5	46·			
11,	43·75	40·25	23,	40·75	41·25			
12,	36·5	44·5	24,	40·75	40·75	Mean corrected,		45°·9

NOVEMBER.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	54·5	56·	13,	39·75	38·25	25,	43·75	43·75
2,	54·5	48·5	14,	30·	31·75	26,	41·75	40·5
3,	50·5	50·75	15,	35·25	38·	27,	33·5	31·25
4,	46·	43·75	16,	45·	42·25	28,	26·5	33·5
5,	50·	45·75	17,	44·5	45·5	29,	38·	38·
6,	45·5	48·5	18,	42·5	44·	30,	32·25	31·
7,	50·25	46·	19,	48·75	45·			
8,	44·5	43·5	20,	45·5	45·	Mean for Month,	44·041	43·758
9,	45·5	53·25	21,	39·	43·			
10,	54·	53·	22,	44·75	48·5			
11,	53·75	49·75	23,	46·5	45·	Mean corrected,		44°·6
12,	45·25	43·	24,	49·75	46·75			

DECEMBER.

	A.M.	P.M.		A.M.	P.M.		A.M.	P.M.
1,	29·	33·75	13,	46·25	44·	25,	41·	44·
2,	40·	41·25	14,	41·25	39·5	26,	48·25	45·75
3,	45·5	47·75	15,	40·75	39·	27,	48·5	42·5
4,	46·	42·5	16,	41·75	39·5	28,	42·75	43·25
5,	50·25	53·5	17,	38·	36·25	29,	44·75	47·5
6,	48·75	54·	18,	31·25	30·25	30,	48·	44·25
7,	36·5	36·	19,	28·	35·	31,	44·	52·25
8,	33·25	32·	20,	30·75	36·5			
9,	31·5	31·25	21,	44·	39·25	Mean for Month,	41·016	41·717
10,	32·5	38·5	22,	42·	43·75			
11,	41·75	47·	23,	45·	46·75			
12,	44·25	42·	24,	46·	44·5	Mean corrected,		42°

Mean Temperature of the year, 48°.

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PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

FIFTIETH SESSION.

5th November, 1851.

The Fiftieth Session of the Philosophical Society of Glasgow was opened this evening.

Walter Crum, Esq., the Vice-President, on taking the chair, expressed his regret at the absence of the President from the opening meeting, and still more at the prospect of his being unable, from advancing years, to take his place in the Society during the Session. He was sure, however, that it would be the unanimous wish of the Society to continue Dr. Thomson as President, notwithstanding his inability to attend its meetings. In these circumstances it had been submitted to the Council this evening, whether it would not be advantageous to the Society to appoint an additional Vice-President, and to allow that office to circulate more than heretofore amongst the members of the Society. On this subject, which he thought it proper to mention to the Society thus early, the Council would probably be in a condition to give a definite opinion at next meeting of the Society.

Mr. James Bryce called attention to the fact, that the Society was now entering upon the 50th year of its existence, and proposed, with the concurrence of several members whom he had consulted, that its jubilee should be celebrated by a dinner, to which eminent men connected with the various sciences should be invited.

Mr. William Brown supported Mr. Bryce's proposal, and suggested that it should be remitted to the Council for consideration; which was unanimously agreed to.

The Librarian mentioned that Mr. Thomas M'Micking had presented to the Society a copy of a flora of the Philippine Islands, written in Spanish by a Spanish Friar, and printed at Manilla, the capital city of the group, and seat of the colonial government.—Thanks voted.

On the motion of Mr. Liddell, it was agreed to request Mr. Cookey and Mr. Dawson to audit the Treasurer's accounts.

Dr. R. D. Thomson, at the request of the President, gave an account

of the American "New Observations on the Winds and Climate of the Atlantic."

XXVI.—The following paper was read last Session, but was not printed in the proceedings:—

Chemical Examination of Drift Weed Kelp from Orkney. By Mr. George William Brown.

Drift weed kelp is derived from the sea weeds which grow on the rocks at the bottom of the Atlantic Ocean. These plants being torn from their native soils by the force of tides and currents, are drifted to the north and north-west coasts of Scotland and Ireland, on which they are thrown by the surge, and being gathered, are burnt either in kilns or in depressions dug in the ground.* By this process most of the organic matter is removed, although in the specimen of kelp investigated and described in this paper, a small portion of carbon and nitrogen still remained. The most important constituents of kelp are the iodine and potash salts. The carbonates were formerly used by the soapmakers, and the insoluble salts for the manufacture of bottle glass.

Previous Analysis.—Although the composition of the kelp salts is well known in a general point of view to the professional chemist, it does not appear, from any experiments which have been recorded, that they have been made the subject of recent minute investigation. Mr. Kirwan, in the end of the last century, published a paper (Memoir read at the Royal Dublin Society and Annales de Chimie, 1793, tom. 18, p. 163,) on *the alkaline substances employed in bleaching linen.* The following is his analysis of what he calls sweet barilha from Spain, which corresponds with kelp in its physical characters:—

Carbonic acid,	16·66
Carbon,	14·95
Lime,	9·42
Magnesia,	2·20
Clay,	2·27
Silica,	4·33
Soda, pure,	14·63
Soda, impure,	4·34
Soda, with common salt,	2·20
Sulphate of soda,	2·17
Chloride of sodium,	1·21
Earthy deposit,	0·34
Water,	25·23

In another volume of the Annales de Chimie, there is a paper by M. Gay Lussac (Annales de Chimie for 1828, tom. 39, pp. 159–163,) on

* History and Description of the kelp manufactory. Proceedings of Glasgow Philosophical Society, vol. ii. p. 241. By C. F. O. Glassford.

"The Potashes of Commerce," in which he gives an analysis of salts of *wareck*—

Sulphate of potash,	22·2
Chloride of potassium,	24·6
Chloride of sodium,	53·2

This appears to be a general statement of the constitution of the soluble salts only.

Dr. Ure, in his Dictionary of the Arts and Manufactures, published in 1840, gives the extremes of his analysis of kelp as follows:—

Soluble salts	Sulphate of soda,	8·0	...	19·0
	Carbonate of soda, And sulphuret of sodium,	8·5	...	5·5
	Muriates of potash and soda,	36·5	...	37·5
	Carbonate of lime,	24·0	...	10·0
	Silica,	8·0	...	0·0
	Alumina, with oxide of iron,	9·0	...	10·0
	Sulphate of lime,	0·0	...	9·5
	Sulphur and loss,	6·0	...	8·5
		100		100

The peculiarity of these results is in the presence of a large quantity of alumina, and the absence of phosphate of lime and alkaline phosphates, which are at variance with the analysis to be described in this paper. The quantity of phosphate of lime in these analysis corresponds with what Dr. Ure terms alumina. It would be difficult to explain the source of such an amount of this earth, as alumina rarely or never enters into the constitution of the vegetable kingdom.

There are analysis of the ashes of some kinds of fuci, viz., F. digitatus, F. vesiculosus, F. nodosus, and F. serratus, in Liebig's Annalen, vol. liv. p. 350, by M. J. Gödechens of Hamburg, published in 1845. The following table gives the calculated mean of these analysis:—

Potash,	11·67
Soda,	12·54
Lime,	11·32
Magnesia,	8·29
Oxide of iron,	0·32
Chloride of sodium,	20·61
Iodide of sodium,	1·33
Sulphuric acid,	19·77
Phosphoric acid,	2·19
Silica,	1·01
Carbonic acid,	5·27
Carbon,	6·05
	100·87

ANALYSIS OF ORKNEY DRIFT WEED KELP.

For the specimen of kelp subjected to examination, I am indebted to W. Paterson, Esq., alkali merchant, Glasgow. The investigation was conducted in the laboratory of the University of Glasgow, under the superintendence of Dr. R. D. Thomson. In making these analyses, the first points to be determined were the quantity of soluble and insoluble salts and water. To effect the first object, a portion of the kelp was digested in water, the solution and residue thrown upon a weighed filter and washed, till all the soluble salts were removed. The insoluble salts were then dried at 212° Fahrenheit:—

	Insoluble salts.	Soluble salts and water.	Insoluble salts, per cent.	Soluble salts and water, p. ct.
400 grains gave.....	114·80 ...	285·20 ...	28·700 ...	71·3
500 " "	155·49 ...	344·51 ...	31·098 ...	68·902
1000 " "	294·10 ...	705·90 ...	29·410 ...	70·59
Mean,.....			29·736 ...	70·264

Water.—The quantity of water was estimated by heating the kelp at 212° Fahrenheit, till it ceased to lose weight.

	Water.	Water, per cent.
200 grains gave	13·60	6·8

If the quantity of water be subtracted from the soluble salts and water, the real amount of soluble salts will be obtained, which is as follows:—

Insoluble salts.	Soluble salts.	Water.
29·069 ...	63·464 ...	6·8

ANALYSIS OF INSOLUBLE SALTS.

The following is the description of analysis and results obtained from the insoluble salts:—

Testing Analysis of Insoluble Salts.

Before proceeding to the quantitative analysis of the insoluble salts, a qualitative investigation was made. The kelp under examination was very hard, with a strong alkaline taste, and greyish colour, with black portions of carbonaceous matter interspersed through it. It was partly soluble in water. That which remained undissolved in water was a greyish powder, which, when ignited, became perfectly white. On addition of acid to the insoluble matter, carbonic acid and sulphuretted hydrogen were evolved, and the greater part of the salts dissolved, that which remained being silica. The portion thus dissolved in acid gave on addition of ammonia, a copious precipitate, which proved on examination to be phosphate of lime, with a trace of iron. To prove the presence of phosphoric acid, it was converted into phosphate of iron by Berthier's method, and the phosphoric acid precipitated as ammonia phosphate of magnesia; the iron being detained by tartaric acid. On the phosphate of lime being separated by filtration, the filtrate gave, with oxalate of

ammonia, a white pulverulent precipitate, proving the presence of lime, which must originally have existed as carbonate or oxysulphuret. After the oxalate of lime had been removed, phosphate of soda and ammonia produced a white crystalline precipitate, indicating the presence of magnesia.

Quantitative Analysis of Insoluble Salts.

Estimation of Organic Matter.—As has been already mentioned, the specimen of kelp under examination had not been entirely freed from nitrogenous matter. This was discovered while deflagrating a portion of the kelp with nitre, when a strong smell of ammonia was given out. At first it was supposed that this might originate from the decomposition of the nitre, but on further investigation it was observed that the kelp, when ignited without the nitre, produced the same odour. A quantitative determination of the nitrogen, hydrogen, and carbon was therefore made.

Estimation of Nitrogen.—The nitrogen was determined in the usual manner, by combustion with soda lime, and passing the ammonia through muriatic acid. The muriate of ammonia thus formed was precipitated by means of the bichloride of platinum, as the yellow ammonia muriate of the bichloride of platinum, which was thrown on a weighed filter washed with alcohol, and dried at 212° Fahrenheit:—

	Yellow salt. NH_3 HCl Pt Cl_2		Nitrogen.		Nitrogen, per cent.
20 grains gave.....	2·1	...	·1317	...	·6585

Carbon and Hydrogen.—To prepare the carbonaceous matter for analysis, 300 grains of the kelp were carefully washed with distilled water, by which process the soluble salts were removed. The matter which was insoluble in water was digested in dilute acid when the insoluble salts were taken up, and organic matter, with silica, remained unacted on. The carbonaceous matter and silica in 300 grains were equal to 14·46 grains. The residue was then subjected to combustion with oxide of copper. The following are the results:—

		Carbon.	Carbon p. ct.
Amount of carbonic acid obtained,	= 10·12 ...	2·76 ...	·920
		Hydrogen.	Hydrogen p. ct.
Amount of water obtained,.....	= 3·47 ...	·433 ...	·144

When the matter insoluble in water was subjected to ignition in a platinum crucible, it lost in weight from the dissipation of the organic matter; but along with organic matter a minute quantity of sulphur and carbonic oxide, from the decomposition of the carbonate of lime, were also driven off, which rendered the results, as far as concerns the organic matter, not strictly accurate:—

	Loss by ignition.		Loss by ignition, per cent.
400 grains gave.....	11·52	...	2·88
500 " "	12·18	...	2·437
500 " "	12·15	...	3·431
Mean,.....			2·582

Estimation of Silica and Sand.—A portion of the kelp was weighed out, and the soluble salts washed out with boiling water. When this was accomplished, the insoluble salts were dried, and the carbonaceous matter removed by ignition; after which they were dissolved in muriatic acid, which took up the insoluble salts, and left the silica and sand. This residue was then boiled with carbonate of soda, which removed the previously combined silica, and the sand remained:—

	Silica and Sand.	Silica.	Sand.	Silica, per cent.	Sand. per cent.
400 grains gave.....	13·24	7·01	6·23	1·75	1·55
400 " "	13·52	7 12	6·40	1·78	1·60
			Mean.....	1·765	1·575

Estimation of Carbonic Acid.—The carbonic acid was determined by introducing the insoluble salts into a flask, from which a tube passed into another flask containing barytes water. The carbonic acid was disengaged by the addition of weak muriatic acid to the salts. The gas passing through the barytes solution yielded a precipitate of carbonate of barytes, which was weighed.

	Carb. of Barytes.	Carbonic Acid.	Carbonic Acid, per cent.
500 grains gave.....	102·05	22·91	4·58

Estimation of Sulphur.—The sulphur was determined by means of the same apparatus as was employed for the estimation of the carbonic acid; but instead of barytes a solution of arsenious acid in caustic soda was used. When the sulphuretted hydrogen was evolved, by means of muriatic acid, it converted the arsenious acid into the tersulphuret of arsenic.

$$As\,O_3 + 3\,SH = As\,S_3 + 3\,HO.$$

The tersulphuret of arsenic was held in solution by the soda, but when muriatic acid was added the yellow tersulphuret fell. This precipitate was then thrown on a weighed filter, and washed with water slightly acidulated with muriatic acid.

	Tersulphuret of Arsenic.	Sulphur.	Sulphur, per cent.
500 grains gave.....	5·16	1·932	·386

Estimation of Phosphate of Lime.—The insoluble salts having been dissolved in acid, and the silica separated, the phosphate of lime was precipitated by ammonia.

	Phosphate of Lime.	Phosphate of Lime, per cent.
400 grains gave.....	42·84	10·71
500 " "	52·50	10·50
500 " "	52·30	10·46
	Mean.....	10·556

Estimation of Alumina.—To ascertain if the phosphate of lime contained alumina, it was dissolved in acid, and then boiled in an excess of strong caustic soda, which would redissolve any alumina. It was found that it contained a small quantity, which was probably accidentally introduced by the caustic soda or other reagents.

	Alumina.	Alumina, per cent.
400 grains gave.....	·740	·185
500 " "	·500	·100
	Mean.....	·1425

Determination of Lime.—To the liquid from which the phosphate of lime had been separated, oxalate of ammonia was added, when oxalate of lime fell. This precipitate being washed and heated to redness was converted into carbonate.

	Carbonate of Lime.	Lime.	Lime, per cent.
500 grains.....	38·85	21·755	4·351

Estimation of Magnesia.—Having removed the lime by filtration, the magnesia was precipitated by means of phosphate of soda and ammonia, as the ammonia phosphate of magnesia, which, when heated, was converted into the diphosphate of magnesia 2 (Mg O) $P O_5$.

	2 (Mg O) P O_5.	Magnesia.	Magnesia, per cent.
500 grains gave.....	43·70	15·607	3·121

The results of the preceding analysis are comprehended in the following table:—

Nitrogen.....	·6585
Hydrogen.....	·144
Carbon.....	·920
Silica.....	1·765
Sand.....	1·575
Carbonic acid.....	4·58
Sulphur.....	·386
Phosphate of lime.....	10·556
Alumina.....	·1425
Lime.....	4·351
Magnesia.....	3·121
	28·1990

		Lime.	Magnesia.	Carbonic Acid.	Phosphoric Acid.	Silica.	Sulphur.
Carbonate of lime..	2·591	1·451	—	1·14	—	—	—
Phosphate of lime..	10·556	5·376	—	—	5·18	—	—
Oxysulphuret of calcium	1·093	·900	—	—	—	—	·386
Silicate of lime.....	3·824	2·059	—	—	—	1·765	—
Carbonate of magnesia	6·554	—	3·121	3·433	—	—	—
Sand.....	1·575						
Carbon.....	·920						
Hydrogen.....	·144						
Nitrogen.....	·658						
Oxygen.....	1·152						
	29·067						

The oxygen was obtained by calculating the quantity necessary to form water, which, being united to the nitrogen, would not be driven off at 212° F.

Analysis of Soluble Salts.

Testing Analysis of Soluble Salts.—Those salts which were soluble in water were, before proceeding to the quantitative analysis, tested qualitatively. The following are the results:—On addition of muriatic acid to the solution of salts, an effervescence took place, with evolution of carbonic acid and sulphuretted hydrogen. Sulphuric acid produced a dark colour in the solution, from the liberation of iodine. This, however, disappeared on heating the liquid, fumes of iodine being evolved. After precipitating the sulphurets by sulphate of copper, the addition of a small quantity of sulphuric acid made the liquid slightly turbid from the precipitation of sulphur, proving the presence of a small quantity of hyposulphurous acid. When nitrate of silver was added to a solution of the salts, a black precipitate fell from the formation of sulphuret of silver; but after a portion of the salts had been boiled with nitric acid, the precipitate with nitrate of silver was white and curdy, indicating the presence of chlorine. Chloride of barium gave a white precipitate, part of which being dissolved with effervescence in nitric acid, indicated the presence of carbonic acid. A white powder remained unacted on by the nitric acid, showing that the salts contained sulphuric acid. After a portion of the salts had been heated to redness, the addition of bichloride of platinum produced a yellow precipitate, proving the existence of potash salts in the kelp. Oxalate of ammonia caused a slight precipitate of lime; and phosphate of soda and ammonia, after some time, a precipitate of ammonia phosphate of magnesia.

Quantitative Analysis of Soluble Salts.

Estimation of Sulphuric Acid.—Having separated, by filtration, a portion of the soluble from the insoluble salts, the sulphuric acid was precipitated in the soluble salts by the addition of chloride of barium and muriatic acid, to dissolve sulphites and phosphates.

	Sulphate of Barytes.		Sulphuric Acid, per cent.
100 grains gave	14·21	...	4·89
100 " "	14·34	...	4·94
Mean per centage,.....			4·915

Estimation of Sulphurous Acid.—To a solution of the soluble salts chloride of barium was added, which precipitated the sulphuric, sulphurous, carbonic, and phosphoric acids, as salts of barytes. This precipitate was thrown on a filter and washed with hot water. The sulphate, sulphite, and carbonate of barytes which were on the filter, were then treated with nitric acid, which converted the sulphite into sulphate, and dissolved the carbonate. The sulphate of barytes was then washed with

water and weighed. The difference between the weight of this precipitate and that of the sulphate of barytes, previously obtained, indicated the amount of sulphate of barytes formed by the action of the nitric acid on the sulphite. From this the sulphurous acid was calculated—

	Sulphate of Barytes.		Sulphuric Acid.		Sulphuric Acid before obtained.		Difference.		Sulphurous Acid.
100 gave	15·75	...	5·40	...	4·915	...	·485	...	·392

Estimation of Hypo-sulphurous Acid.—The soluble salts being separated, by means of cold water, from the insoluble salts; the sulphurets and carbonates were removed by sulphate of copper. After separating this precipitate by filtration, sulphuric acid was added to the liquid, which decomposed the hypo-sulphites, sulphurous acid being evolved and sulphur precipitated. This sulphur was then washed, dried at 212° F., and weighed.

	Sulphur.		Hypo-sulphurous Acid.		Hypo-sulphurous Acid, Per Cent.
400 grains gave.....	·18	=	·54	...	·115

Estimation of Sulphur.—The quantity of sulphur in the soluble salts was estimated by deflagrating a portion of the kelp with nitre. By this process all the sulphites, hypo-sulphites, and sulphurets, were converted into sulphates. The sulphuric acid was then precipitated by chloride of barium as sulphate of barytes, which was weighed, and the sulphuric acid contained in it calculated. It is obvious that the sulphuric acid thus obtained, comprehended all the sulphur existing as sulphurets and sulphur acids in the original kelp. If we subtract from it the sulphuric acid found to exist, as such, in the kelp, we have remaining the sulphuric acid equivalent to the sulphites, hypo-sulphites, and sulphurets in the kelp. If, again, we subtract from the last result the calculated quantity of sulphuric acid equivalent to the sulphurous, hypo-sulphurous acid, and the sulphuret of the insoluble salts, the remainder will be the sulphuric acid equivalent to the sulphur of the soluble salts.

	Sulphate of Barytes.	Sulphuric Acid.	Sulphuric Acid, per cent.
200 grains gave	46·55	16·04	8·02
Original sulphuric acid,.....			4·92
Sulphuric acid = Sulphurous acid, ...		·490 ...	3·10
— — = Hypo-sulphurous acid,.....		·191	
— — = Sulphur of insoluble salts,..		·965	———
Total calculated sulphuric acid,.....			1.446
Sulphuric acid = Sulphur of soluble salts, ...			1·654
Sulphur, soluble salts, per cent.,.....			·6616

Estimation of Phosphoric Acid.—To the solution from which the sulphuric acid had been precipitated, by chloride of barium and muriatic acid, after separation of the precipitate, ammonia was added, when phosphate of barytes fell.

	Phosphate of Barytes.		Phosphoric Acid.		Phosphoric Acid, per cent.
200 grains gave	2·02		·649		·3245

Estimation of Carbonic Acid.—The carbonic acid in the soluble salts was determined in the same manner as in the insoluble salts, by passing the gas evolved by the muriatic acid from the solution of the salts, through caustic barytes dissolved in water. The carbonic acid precipitated the barytes as carbonate, from which the carbonic acid was calculated.

	Carbonate of Barytes.	Carbonic Acid.	Carbonic Acid, per cent.
500 grains gave	48·62	10·9	2·180

Estimation of Chlorine.—A solution of the soluble salts was boiled with nitric acid, to convert the sulphurets into sulphates; the chlorine was then precipitated by nitrate of silver.

	Chloride of Silver.	Chlorine.	Chlorine, per cent.
15 grains gave	14·1	3·52	23·40
— —	15·5	3·88	25·33
	Mean,.....		24·365

Estimation of Iodine.—This, which is one of the most valuable constituents of kelp, was determined by the following method, which has yielded results very satisfactory.

A portion of the kelp was exhausted of its iodide, by digestion *several times* in alcohol. The alcoholic solution was then evaporated to dryness, and, to convert any sulphuret which might have been taken up by alcohol into sulphate, was deflagrated with chlorate of potash, and kept at a red heat till any iodate that might have been formed by the action of the chlorate of potash, was decomposed. The mass was then dissolved in water, and the iodine precipitated, by means of chloride of palladium, as iodide of palladium, which was dried at 212° F., and weighed—

	Iodide of Palladium.	Iodine.	Iodine, per cent.
1000 grains gave	4·05	2·83	·283
— —	4·35	3·06	·306
500 —	2·05	1·437.....	·287
	Mean Iodine, per Cent,.....		·292

Separation of Bromine and Iodine.—To effect the separation of the iodine and bromine, a pound of kelp was treated with alcohol which dissolved out the bromide and iodide. The alcohol was then driven off. Through the aqueous solution of the salts, chlorine was passed in order to decompose the iodide and bromide, the iodine and bromine being set free. This liquor holding in solution free iodine and bromine, was frequently agitated with ether in a stoppered bottle. The aqueous solution gradually became clear on standing, and the ether, containing the bromine and iodine, floated on the surface. This ethereal solution was then decanted and saturated with soda, after which it was evaporated to dryness and heated to redness, to destroy any iodate or bromate. The residual salts were dissolved in water, and the iodine precipitated by

chloride of palladium. The iodide of palladium being separated by filtration, the excess of palladium was removed from the filtrate by sulphohydret of ammonia. It was found in this experiment that sulphohydret of ammonia answered better than sulphohydric acid for removing the excess of palladium; because, when sulphohydric acid is employed, part of the sulphuret of palladium is dissolved by the acid, which was previously united to the palladium, which was set free by the sulphohydric acid. Having removed the excess of sulphohydret of ammonia by boiling, chlorine was again passed through the solution to decompose bromide. The bromine which was set free was taken up by ether (this had a yellow colour, probably from the presence of a small quantity of bromine). The ethereal solution was then neutralized by soda, evaporated to dryness, and heated to redness. The aqueous solution of the residue gave a white precipitate with nitrate of silver, which consisted principally of chloride of silver. But from the colour of the ether, it was evident that it contained a small quantity of bromine.

Estimation of Potassium.—To determine accurately the quantity of potash, it was considered advisable to convert any potash that might exist as sulphate into chloride, which was effected in the following manner. From the solution of the salts, the sulphuric acid was precipitated by chloride of barium and the sulphate of barytes, separated by filtration. The excess of barytes was then thrown down by carbonate of ammonia. The liquor, after the carbonate of barytes had been removed, was evaporated to dryness, and heated to redness to expel the ammonia. The residue was dissolved in water, and the potassium precipitated by the addition of the sodium bichloride of platinum, as the potassium bichloride of platinum ($KCl\ Pt\ Cl_2$).

	($KCl\ Pt\ Cl_2$)	Potassium.	Potassium, per cent.
30 grains gave.....	31·0 ...	5·071 ...	16·

Estimation of Lime.—This was determined by precipitation as oxalate of lime; the precipitate, when heated, was converted into carbonate of lime.

	Carbonate of Lime.	Lime.	Lime, per cent.
400 grains gave.....	1·49 ...	·805 ...	·201
500 " "	2·33 ...	1·300 ...	·260
Mean lime per cent.....			·230

Estimation of Magnesia.—The magnesia was precipitated from the solution of the salts, by phosphate of soda and ammonia, as ammonia phosphate of magnesia, which was converted, by heat, into the diphosphate of magnesia.

	Diphosphate of Magnesia.	Magnesia.	Magnesia, per cent.
500 grains gave	3·61 ...	1·321 ...	·264
400 " "	3·00 ...	1·071 ...	·267
Mean magnesia, per cent			·277

Results of Analysis of Soluble Salts.

Sulphuric acid	4·915
Sulphurous acid	·392
Hypo-sulphurous acid	·135
Sulphur	·6616
Phosphoric acid	·3245
Carbonic acid	2·162
Chlorine	24·365
Iodine	·292
Bromine	trace.
Potassium	16·000
Lime	·230
Magnesia	·271
	49·754

The deficiency here is the soda, as may be seen in the two following tables in which the acids and bases are united according to their affinities; and the result of the examination of a hundred parts of the kelp is given, comprehending both soluble and insoluble matter:—

	Sulphuric Acid	Sulphurous Acid	Hyposulphurous Acid	Sulphur	Chlorine	Potassium	Sodium	Lime	Magnesia
Sulphate of potash,	2·058	—	—	—	—	2·057	—	—	—
Sulphate of soda,	2·000	—	—	—	—	—	1·06	—	—
Sulphate of lime,	0·164	—	—	—	—	—	—	·115	—
Sulphate of magnes.	·693	—	—	—	—	—	—	—	·281
Sulphite of soda,	—	·392	—	—	—	—	·261	—	—
Hyposulphite of soda,	—	—	·132	—	—	—	·058	—	—
Sulphuret of sodium,	—	—	—	·6616	—	—	·9894	—	—
	Phosphoric acid.								
Phosphate of soda,	—	·3245	—	—	—	—	·162	—	—
	Carbonic acid.								
Carbonate of soda,	—	2·180	—	—	—	—	1·846	—	—
Chloride of potassium	—	—	—	—	12·584	13·943	—	—	—
Chloride of sodium,	—	—	—	—	11·570	—	7·764	—	—
Chloride of calcium,	—	—	—	—	·147	—	—	·116	—
	Iodine.								
Iodide of magnesium	—	·292	—	—	—	—	—	—	·040
Bromide of magnesium, trace.									

TABLE OF PER CENTAGE COMPOSITION OF ORKNEY KELP.

Insoluble Salts.

Carbonate of lime	2·591
Phosphate of lime	10·556
Oxysulphuret of calcium, 3 CaS, CaO	1·093
Silicate of lime	3·824
Carbonate of magnesia	6·554
Sand	1·575

Alumina,	·142	
Carbon,	·920	
Hydrogen,	·144	
Oxygen,	1·152	
Nitrogen,	·658	29·209
Soluble Salts.		
Sulphate of potash,	4·527	
Sulphate of soda,	3·600	
Sulphate of lime,	·279	
Sulphate of magnesia,	·924	
Sulphite of soda,	·784	
Hyposulphite of soda,	·220	
Sulphuret of sodium,	1·651	
Phosphate of soda,	·540	
Carbonate of soda,	5·306	
Chloride of potassium,	26·491	
Chloride of sodium,	19·334	
Chloride of calcium,	·229	
Iodide of magnesium,	·316	
Bromide of magnesium,	trace.	
Water,	6·800	71·001
	100·210	

November 19, 1851.—Mr. Crum *in the Chair.*

The following were elected members, viz.:—Rev. Dr. Robson, Rev. Dr. Memes, (Hamilton,) Messrs. John Thomson, civil engineer, William M. Thomson, William Wright.

The following is the substance of a report read by the Librarian on the state of the library:—

Every volume in the library has been inspected, and found to be in good order. The missing volume of Whewell's Inductive Sciences, vol. 1, has been recovered. The number of vols. in the library was, on the 1st of November, 1851, as follows:—

French periodicals, ...	308 vols.
English periodicals, ...	537
German periodicals, with 60 French vols, .	317
Societies' Journals, ...	234
General science, ...	229
Encyclopædias, &c., ...	295
Total in 1851, . . .	1920
Total in 1850, . . .	1683
Increase, ...	237

The number of periodicals is as follows:—

English.		French.		German.		
Weekly,	1	Weekly,	2	Weekly,	1	
Fortnightly,	1	—		—		
Monthly,	9	Monthly,	5	Monthly,	3	
Bimonthly,	1	—		—		
Quarterly,	5	Quarterly,	2	—		
Annually,	13	Annually,	1	—		
Total,	30		10		4	—Total, 44.

TABLE OF THE NUMBER OF VOLUMES ISSUED MONTHLY, AND NUMBER OF READERS MONTHLY.

	1850.			1851.		
	No. of Vols.		No. of Readers.	No. of Vols.		No. of Readers.
January,	—		—	127	...	42
February,	20	...	11	116	...	44
March,	108	...	40	87	...	34
April,	123	...	45	157	...	45
May,	91	...	31	138	...	46
June,	79	...	31	120	...	38
July,	94	...	33	94	...	35
August,	87	...	28	112	...	37
September,	85	...	34	150	...	41
October,	72	...	35	140	...	44
November,	118	...	48	162	...	45
December,	118	...	44	147	...	50

The mean number of volumes out each day of the month of
September, 1851, was 70·6
October, 1851, " 85·

Since last report, the abridgment of the Philosophical Transactions, from 1665 to 1837, in 21 volumes, and the Transactions of the Society of Arts, from 1806 to 1832, in 49 volumes, have been added to the library, in furtherance of the project of completing important series of works annually.

A supplement to the library catalogue has been printed, containing all the additions during the last two years, up to the present time.

Abstract of Treasurer's Account.

Mr. Liddell, the Treasurer, gave in the following Abstract of his Account for Session 1850–51:—

1850. DR.

Nov. 1.—To Cash in Union and Savings Bank,..£90 3 4

1851.

Nov. 1. — Interest on do. 3 5 3

£93 8 7

Nov. 1.—To Society's "Transactions" sold,.....					4	16 0
— Entries of 38 New Members, at 21s. each,.....	39	18	0			
— 12 Annual Payments from Original Members, at 5s. each,.....	3	0	0			
— 270 Annual Payments at 15s. each,	202	10	0			
				245	8	0
				£343	12	7

1851. Cr.

Nov. 1.—By New Books and Binding,.....				£93	4	4
— Printing Transactions, Circulars, &c.....				33	7	0
— Gas Fittings, &c., in Hall,.....				1	19	0
— Rent of Hall,.....	£15	0	0			
— Coffee, &c., at Annual Meeting,...	2	15	0			
— Fire Insurance,.....	2	16	0			
— Society's Officer, Clerk, and Poundage on dues collected,...	13	1	6			
— Postages, Delivering Letters, and Stationery,.....	14	17	6			
— Gas for Hall, &c.....	0	13	0			
				49	3	0
— Librarian's Salary (50½ weeks),.....				18	0	0
— Subscription to Ray Society, 1 yr.	1	1	0			
— Do. to Cavendish Society, 1 yr...	1	1	0			
— Do. to Palæontographical Society, 2 years,.....	2	2	0			
— Donation for investigating the Local Natural History,.....	15	0	0			
				19	4	0
— Cash in Union Bank,.....	120	0	0			
— Cash in Savings Bank,.....	8	15	3			
				128	15	3
				£343	12	7

THE PHILOSOPHICAL SOCIETY EXHIBITION FUND.

1850.				
May. 15.—To balance, as per deposit Receipt from the Corporation of the City of Glasgow,.....			£529	16 9
1851.				
May 15.—To one year's Interest on do.,.....			21	4 0
			£551	0 9

GLASGOW, *3d November*, 1851.—We have examined the Treasurer's Account, and compared the same with the Vouchers, and find that there are in the Union Bank of Scotland One Hundred and Twenty Pounds, and in the Savings Bank Eight Pounds Fifteen Shillings and Threepence—together, One Hundred and Twenty-Eight Pounds Fifteen Shillings and Threepence at the Society's credit. The Treasurer has also exhibited to us a Voucher, which he holds for money lent to the Corporation of the City of Glasgow, from the proceeds of the Philosophical Society's Exhibition in 1846, with interest thereon up to 15th May ult., being £551 0s. 9d.

THOMAS DAWSON.
WM. COCKEY.

Report by Treasurer, 5th November, 1851.—The property possessed by the Society at this date consists of the above-named balance of £128 15s. 3d. in Bank; the Books in Library, and Book Presses, as per Librarian's Catalogue. The Furniture, Picture, Bust, &c., remain same as in Report of last year.

The number of new Members admitted Session 1850–51, is 38. From this there fall to be deducted 13 dropped from the Roll, viz., for arrear of dues, 4; placed on non-resident list, having removed from Glasgow, dues being paid to date, and intimation to be placed on this list being given, 2; resigned membership by letter, 4; dead, 2; for non-payment of entry-money and first year's dues, 1. The total number on the Roll at this date is 286; of these 10 are in arrear of dues for one year.

THE LATE MR. JOHN HART.

MR. ANDREW LIDDELL, the Treasurer, in reporting on the deaths during the past year, referred to that of Mr. John Hart, one of the original members of the Society, and begged permission to make a short statement respecting him.

He asked this not only because of Mr. Hart having been for so many years (nearly thirty) connected with this Institution, and for the greater portion of these a very active member, but also and chiefly because, in the history of the deceased, we have a brilliant specimen of the effects of self-culture, in a comparatively unlettered youth acquiring a highly cultivated mind, a thorough knowledge of many branches of science, becoming a good worker in several branches of handicraft, and a promoter of schools and institutions like our own, that taught to others the same things.

The deceased was a careful observer of all new inventions in mechanics, and of all new discoveries or theories in philosophy. Many of these he brought to the test of experiment in the laboratory and workshop which adjoined his bakery in Mitchell-Street. By practice he acquired considerable proficiency in wood and metal turning, as also in other branches of mechanical art: he made working models of steam-engines, optical instruments, including large sized telescopes, with various other machines and instruments. The turning lathes in his workshop were moved by a steam-engine of his own making. The parts of that engine, which required to be bored or turned, were done by power from a water-wheel which he had previously set down in the course of St. Enoch Burn, which runs close by his premises.

Mr. Hart was a native of Borrowstouness; born 18th August, 1783, died 27th June, 1851; was consequently in his 68th year. It may be noticed that his natal day was that on which the great meteor or fire-ball traversed the horizon of Great Britain. After getting the school education usually given in a country town at that date, he was for some time employed by a timber merchant in his native place as an interim clerk. When about seventeen years of age, he removed to Glasgow in search of similar employment, but not finding a suitable place, he learned the trade of a pastry-baker with an uncle. His brother Robert followed soon after, and also learned the same trade. The two brothers ultimately succeeded their uncle in his business, in which they continued till 1831. At this date both retired from all business pursuits. It may be here mentioned, what indeed is known to many present, that Mr. Robert Hart was not only a co-partner in business, but was also a hearty co-operator with the deceased in all his learned and scientific pursuits. That gentleman is still amongst us, and, it is hoped, will long continue to prosecute the useful arts which he and his brother so creditably pursued for many years.

The only instruction in philosophy or the arts which the deceased got was in the Mechanics' Class of Anderson's University, he all the time following his business of pastry-baking. Feeling the great advantage which he derived from this source, he at an early period took an active part in the management of that class for the good of others, and not only of that particular class, but also of all the concerns of Anderson's University. For about forty years, and up to the period of his death, he was a trustee and a member of the Managing Committee of that Institution, and he has been a member of this Society for nearly thirty years. In addition to philosophy and mechanics, the deceased had a taste for, and a considerable knowledge of, the fine arts. He was a good draughtsman, and excelled in the art of modelling. We have a good specimen of his work in this line in the full-sized statue of the late Professor Anderson in the museum of the University. His great knowledge of many of the sciences and of the mechanic arts, his being also of easy access, modest, and unassuming, and withal ever ready to communicate, encouraged his friends and the citizens generally to draw upon him for information. This was done frequently for their guidance in applying the information thus acquired to the arts and manufactures in which they were respectively engaged. And it is believed that much useful knowledge was got in this way. In his workshop, or laboratory, were to be seen or heard of, on all occasions, almost every thing that was new in science or art. At one time you might see, in a half finished state, a tiny steam-engine, the speculum and other parts of a large sized telescope, also in a half finished state, and perhaps the plan of a gas apparatus. On your return you might find the engine driving his turning-lathes, the telescope on its pedestal, and the gas apparatus brilliantly illuminating all his premises. This was the case prior to the operations of the Gas Company in 1818, for Messrs. Harts' premises alone were lighted with gas at that date. As

a consequence, they were visited by many of the citizens, and not by them only, but also by many scientific strangers. Amongst the latter may be mentioned the late James Watt, who, on every visit he made to Glasgow, uniformly called on the deceased and his brother, and frequently invited them to spend evenings with him at his hotel. Shortly after his return from the last visit he made to Glasgow, he, as a mark of his regard, presented the Messrs. Hart with some valuable work tools, and accompanied the present with a letter, of which the following is a copy:—

"Heathfield, Birmingham, December 19th, 1815.

"Gentlemen,—On Saturday last, I took the liberty of sending, by the Manchester waggon for Glasgow, a small box, directed as this letter, containing a best Sheffield brace, and thirty-eight bits, and two drill stocks, with twelve drills each, of which I request your acceptance, as a mark of my regard. I hope they will be of use in your pursuits.

"They may be in Glasgow in a fortnight, and you may inquire for the box at Mrs. Walsh's, in Stirling Square.

"I shall be glad to hear that you receive them safe, and how your telescope goes on. And remain with esteem,

"Gentlemen,

"Your obedient humble servant,

(Signed) "James Watt.

"Messrs. J. & R. Hart,
Mitchell-Street, Glasgow."

The Society then proceeded to the Fiftieth Annual Election of Office-Bearers. Mr. Cockey and Mr. Dawson were requested to act as scrutineers of the votes. The scrutineers having retired to examine the vote-papers,

Professor William Thomson read a paper "On the Decomposition of Water by Galvanic Elements of low intensity, and on the Electro-Polarization of Platinum Plates."

The scrutineers reported the following to be the result of the election:—

President.

Thomas Thomson, M.D.

Vice-President,..Walter Crum. | Librarian, ..R. D. Thomson, M.D.
Treasurer,.....Andrew Liddell. |

Joint Secretaries.

Alexander Hastie, M.P. | William Keddie.

Council.

James Bryce, M.A.	Arthur Mitchell, M.D.	William Thomson, M.A.
William Ferguson.	William Murray.	Allen Thomson, M.D.
William Gourlie.	Neil Robson.	G. W. Arnott, LL.D.
Alexander Harvey.	Robert Sinclair.	A. K. Young, M.D.

December 3d, 1851.—*In the absence of the* PRESIDENT *and* VICE-PRESIDENT, MR. LIDDELL *was called to the Chair.*

THE following were elected members, viz.:—Messrs. Nathaniel Holmes, William Wallace, Robert Bruce Bell, Daniel Miller, George Glennie, William Thomson, Ramsay Thomson.

The Society voted a second time on the proposal submitted by the Council that an additional Vice-President should be appointed, when it was unanimously and finally approved of. It was in like manner agreed that neither of the Vice-Presidents shall be eligible for more than two years consecutively, and that the election to the office shall be annual, as in the case of the Office-bearers.

Dr. G. A. Walker Arnott was elected Vice-President.

It was resolved that at next meeting the Society should elect a member of council in room of Dr. Walker Arnott.

Mr. Robert Blackie gave notice of a motion to the following effect:—

"That instead of the entire council of this Society going out of office annually, and being eligible at once for re-election, only four members of council shall retire annually, who shall not be eligible for re-election till they have been one year out of office, and that four new members be elected annually in room of those that go out.

"That this change be brought into operation at next general election, in the following manner:—The names of the twelve councillors then elected to be arranged in accordance with the number of votes recorded for each,—the one having the greatest number of votes to be placed at the bottom of the list, that he may remain longest in office, and the others progressively above—the one having the least number of votes being placed at the top. That at the end of one year the four councillors whose names are at the top of the list, retire from office, and four new ones be elected, and their names placed at the bottom, and so on in succession."

Mr. John Thomson, C.E., read "Remarks on the Advantages of Tubular Drainage, as applied to Houses, Streets, and Towns."

December 17th, 1851.—*In the absence of both the* VICE-PRESIDENTS, MR. WILLIAM MURRAY *was called to the Chair.*

THE following were elected members, viz:—Lord John Hay, Commander, R.N.; Messrs. Thomas Carlile, Andrew Kelley, Robert Stark.

Mr. Hastie stated that Mr. Blackie's motion for a change in the mode of electing the members of council, had this evening been under the consideration of the Council, by whom a report on the proposal would be submitted to the Society before the close of the Session.

The Society proceeded to elect a member to fill the vacancy in the Council, caused by the appointment of Dr. George Walker Arnott to the

office of Vice-President. Mr. Ramsay and Mr. King were requested to scrutinize the votes; which having been done, it was found that the election was in favour of Mr. William Cockey.

Mr. James Bryce stated his views respecting "The Dispersion of Granite Boulders in the South Highlands, in reply to a late paper by the President of the Geological Society."

The following paper was read:—

XXVII.—*Notes on the Introduction of the Potato into Scotland.* By John Scouler, M.D., LL.D., F.L.S.

[Communicated by William Gourlie, Esq.]

The following notes respecting the progress of the culture of the Potato, although very incomplete, will, I trust, be of some interest to the Society. It is a common but certainly a most erroneous opinion, that the potato was first introduced into Ireland by Sir W. Raleigh, and that from thence it was disseminated over the rest of Europe, under the name of the Virginian potato. The following remarks will, however, exhibit the fallacy of this opinion. The different esculent roots, the produce of America, were confounded by the older writers, and thus it is often difficult to ascertain the particular species they intended to describe; in consulting their works we must distinguish between the (*Helianthus tuberosus*), Canada potato or Artichoke, the Batata or sweet potato, and the papa or true potato. Bearing this distinction in mind, we may state that there is no evidence of a satisfactory kind that the true potato was a native of North America; we find no notice of it in the early history of Virginia, although the first colonists often suffered from famine, and consequently had a deep interest in ascertaining what were the resources of the country. In like manner, although we have copious information of every thing relating to Mexico, in the narratives of the conquerors, we never find the potato mentioned. As respects Peru and Quito the case is very different, and we have early and authentic information respecting the potato. In the work of Cieca de Leon, published in 1554, the culture of the potato is described, he expressly distinguishes the "batatas dulces" or sweet potatoes from the true potato, called by the Peruvians papa, and he informs us that in the more elevated regions of the Andes, where the maize did not ripen, the *papa* or potato was an important article of food. Whatever opinion we may form respecting the native country of the potato, it is certain that the merit of importing it into Europe does not belong to Raleigh. As a convincing proof that the potato was cultivated in Europe previous to the voyage of Raleigh we may, with Parmentier, quote the evidence of Clusius, in his work entitled *Rariorum Plantarum Historia*, and printed in the year 1601. Clusius informs us that in the year 1586, the potato was in common use in Italy, not only as food for men but also for cattle. Raleigh returned from his voyage in the year 1585. In addition to this evidence we may cite that of Matthiolus, which is equally explicit. In his edition of *Dioscorides*,

published in 1598, we have an excellent wood engraving of the potato plant, and its culture appears to have been well understood on the continent for several years before that date—the following is his statement:—"hyberno tempore radices eximebantur ne putrescerent vernalique tempore rursus terræ committebantur, ast nunc non item, si quidem tot se tuberibus propaget, ita ut ad unam plantam hiberno tempore eratam ultra quadraginta tubera notarem, quamvis etiam ex rarimis reclinatis terra tectis Burgundi propagare solent." It is right, however, to add, that he states the potato was introduced by the English from Virginia. In whatever way this notion may have arisen, we have already seen that it had been well known in Italy before the voyage of Raleigh to America. The names by which it was known on the continent seem to point out that it was transmitted from Spain to Italy, and from thence to Germany, thus the Spaniards in Peru gave it the name of "turmas de tierra" or truffles, and hence the Italian "tartuffoli," and the German "kartoffeln."

Although the potato was cultivated on the continent previous to its introduction into Ireland, yet it was in the latter country that its importance was first appreciated. The earliest notice with which I am acquainted respecting the general culture of the potato occurs in the *Political Anatomy of Ireland*, by Sir William Petty, which was published in 1672. He remarks, speaking of the Irish, "their laziness seems to me to proceed rather from want of employment and encouragement to work, than from natural abundance of flegm in their bowels and blood; for what need they to work who can content themselves with potatoes, whereof so the labour of one man can feed forty; and with milk, whereof one cow, in summer time, will give meat and drink enough for three men; and when they can every where gather cockles, oysters, mussels, crabs, &c.; with boats, nets, and angles, or the art of fishing; and can build a house in three days." From this time the use, or rather abuse of the potato, became more and more general in Ireland, and Threlkeld, in his *Irish Herbal*, after a quaint panegyric on the potato, observes "dearth of bread can never affect us much while this crop answers as it has done this year, 1725."

The progress of the culture of the potato was both later and slower in Scotland than in Ireland, and it seems to have spread from west to east, that is from those parts of the coast opposite Ireland. The earliest notice of the plant by any Scottish writer, is in *Sutherland's Catalogue* (1683), of plants cultivated in the Edinburgh Botanic Garden. In this case it is to be considered as a botanical curiosity rather than as a plant of any economical importance. The earliest notice with which I am acquainted occurs in *Martin's Western Islands* (1703), from which it appears that they constituted, even then, an important article of food to the Islanders, and also renders it probable that they had been originally imported from Ireland. It was many years after their introduction into the Western Islands before they became an object of attention to the

Lowland farmer. In the *Countryman's Rudiments*, 1723, we have the following advice to the farmers from Lord Belhaven, "for roots I advise you to sow potatoes and turnips, a larger or lesser quantity as you affect most, but rather potatoes, because being once planted they will never fail, they require little more labour than to keep the ground where they grow free from grass. The Flandrian boors make so much of this root, and had such plenty thereof, that both the Confederates and French army found great support thereby, by feeding the common soldiers most plenteously; it is both delicious and wholesome." (p. 33.)

It appears, however, that notwithstanding this advice, founded upon observations which prove that the potato was extensively cultivated in the low country, that it was unknown in the east of Scotland, while its value was gradually becoming known in the districts adjacent to Ireland and the Western Islands. It appears from *Stewart's Trial*, 1753, that they were generally cultivated in Argyleshire, especially in the neighbourhood of Glencoe. The earliest notice I find of their introduction into Ayrshire, occurs in *Robertson's Rural Recollections*, in which he says, "from an old lease which I have seen, dated in 1729, between John Montgomery the proprietor and his tenant William Liddle, of the lands of Broadlee, in the parish of Dalry, it is stipulated that the tenant shall allow the laird to cultivate a certain quantity of land yearly for potatoes so far as he can find dung." The progress of the potato cultivation must have been very slow, for the same author informs us that in the year 1733, the potato was served up for supper several times in the Eglinton family. It would appear, therefore, that the potato was considered rather as a garden vegetable than as an important crop for the farmer. If the narration in the statistical account of the parish of Kilsyth be correct, the merit of introducing the potato into general use belongs to Robert Graham, of Tamrawer, in that parish. It is stated that before 1739, he and others had raised them in gardens, but it was a common prejudice that they could not be raised any where else to advantage. Mr. Graham, to show the absurdity of this opinion, planted about half an acre of ground in the croft of Neilston, where he then resided; the report of it soon spread far and wide, insomuch that people of all denominations, and some noblemen of the highest rank, among whom was the Earl of Perth, came to visit the plantation; and further, he rented lands in the vicinity of the towns of Renfrew, Perth, Dundee, Glasgow, and Edinburgh, on speculation, and for many years he obtained the premium for cultivating potatoes.

In accordance with these statements, it appears that previous to 1746, Glasgow was supplied with potatoes from the Highlands, at all events water-borne, and subsequently the city began to obtain supplies from the surrounding districts. The following extract from the *Glasgow Journal* (271), gives us much curious information respecting the spread of the potato, which is contained in a proclamation by the magistrates regulating the sale of that vegetable. "Oct. 6th, 1746.—Whereas the

inhabitants have been in the use to be served with potatoes from the coast, water-borne, and sold with the water measure; and that now of late potatoes are planted in the neighbourhood, and brought into the city for sale by country people who use different measures, and less than the water measure, and make thus mercat in many parts of the streets, and before shop doors, and at the entry of closes; and by these different measures the inhabitants are imposed upon, and it being judged proper that the place be ascertained where the potatoes are to be brought to for sale, and all to be sold for the water measure; therefore the magistrates enact, that from and after the 8th October instant, the mercat-place for the sale of potatoes be at the entry of the Candleriggs from the Trongate, upon both sides of the street, to which place the inhabitants are to repair; except as to potatoes sold at the Bromielaw, water-borne, and that no mercat be made upon any of the parts of the streets, or entries of closes, but the place above ascertained, and that all potatoes, whether water-borne or brought into the city from the country for sale, be sold by the water measure, at the weighhouse, and no other measure certifying the contraveners hereof, both sellers and buyers, that they will be fined, punished, and the potatoes confiscate."

January 21, 1852.—Dr. Walker Arnott *in the Chair.*

Letters were produced from the Secretaries of the Royal Institution of Great Britain, the Royal Society of London, and the Geological Society of London, acknowledging receipt of the last Number of the printed Proceedings of this Society.

Mr. Paul Cameron read a paper on the "Properties of Steam, and the best Means of calculating its Power." He afterwards exhibited a new salineometer.

February 4.—Mr. Crum *in the Chair.*

Mr. W. J. Macquorn Rankine was elected a member.

The following Minute of Council was read:—

"*February* 4.—The Council having resumed consideration of the proposal made at last meeting by Mr. Murray, that the Society shall invite the British Association for the Advancement of Science to Glasgow, Mr. Liddell produced the draft of a memorial to the Lord Provost, Magistrates, and Town Council, requesting their concurrence in the invitation; which memorial was approved of, and Mr. Liddell was authorized to bring the subject before the Society this evening."

Mr. Liddell, after some explanatory remarks, moved the adoption of the following Memorial:—

"Unto the Honourable the Lord Provost, Magistrates, and Council of the City of Glasgow. The Memorial of the President, Vice-Presidents, and Members of the Glasgow Philosophical Society,

"Sheweth,

"That, in the year 1838, the then Magistrates and Council, at the suggestion of the Philosophical Society, were pleased to forward an invitation to the British Association for the Advancement of Science, to hold its annual meeting in 1839 in Glasgow, or if not in that year, at as early a period thereafter as possible. And that at the suggestion of the Magistrates and Council, the Principal and Professors of the University, and the other public bodies in the city, were severally pleased to send invitations to the same effect.

"The simultaneous request from the public bodies in Glasgow, thus so cordially made, was responded to by the Association, and its meeting in 1840 was held in our city. It may be recollected that at that meeting were congregated not only the members of the Association, natives of Britain, but also nearly one hundred foreigners, including many names of the highest respect in Europe and America, a greater number than have been present at any other meeting of the Association.

"Considering the many advantages that accrued to Glasgow from the past, and that similar may be expected to flow from another, visit of that distinguished body,

"Your memorialists are of opinion that the time has arrived when the British Association should receive a cordial invitation to hold its meeting in Glasgow in 1853, or in an early year thereafter.

"They therefore respectfully solicit the Lord Provost, Magistrates, and Council to present an invitation to that effect to the Association at its next meeting—which is to be in Belfast in the course of next summer—and to take such steps as may seem most expedient, in order that the Principal and Professors of the University, and the heads of the public bodies, may be induced to co-operate, and forward at the same time similar invitations.

"The Lord Provost and Magistrates may depend on the cordial concurrence of the Philosophical Society in this very important measure.

"Signed by the office-bearers in name and by appointment of the Glasgow Philosophical Society, this twenty-sixth day of February, Eighteen Hundred and Fifty-two."

The motion was seconded by Sir James Anderson, who stated that it would afford him pleasure to support the memorial in the Town Council.

Mr. Murray mentioned that the Lord Provost had requested him to say that he regretted being unable to be present in the Society to-night to support this motion, to which he had no doubt his coadjutors in the Magistracy and Town Council would agree.

The Memorial was then approved of.

Professor William Thomson read a paper "On the Thermo-Electric Properties of Platinum, Copper, and Iron."

February 18, 1852.—MR. CRUM *in the Chair.*

MR. WILLIAM HARVEY, junior, and Mr. Charles Wilson, were elected members.

Dr. Penny read a paper on "The Chemical Analysis of Commercial Salts of Potash."

Mr. Daniel Miller, C.E., described his "Patent Hydraulic Purchase Machinery," for drawing up Ships on Slip Docks, and "Hydraulic Quadrant Dock."

March 3, 1852.—MR. CRUM *in the Chair.*

MR. ALEXANDER HARVEY reported that the Magistrates and Town Council had unanimously concurred with the Philosophical Society in the proposal to invite the British Association for the Advancement of Science to revisit Glasgow, and had appointed a committee in furtherance of the object.

It was moved by Mr. Liddell, seconded by Dr. Walker Arnott, and agreed, to remit to the Council to prepare an invitation to the British Association for the Advancement of Science to hold its meeting in Glasgow in 1853, or at the earliest period convenient to it; and also to select a deputation from this Society to lay the invitation before the Association at its meeting in Belfast during the present year; and further authorize the Council, or a committee of it, to co-operate with the Lord Provost, Magistrates, and Committee of Town Council, and with the other public bodies in the city who propose to send similar invitations.

Mr. J. Napier read the following paper:—

XXVIII.—*Remarks upon Mineral Veins and Water-Worn Stones.* By JAMES NAPIER, ESQ., F.C.S.

IN the crust of the earth are found a great many cracks or fissures varying in size from a few inches to hundreds of feet in breadth, and of a length and depth unknown. These cracks are supposed by some to be the results of an internal upheaving pressure, by others, of a magnetic current.

A great number of these fissures have become wholly or partially filled with various kinds of minerals, generally of a crystalline character, and often distinct from any composing the rocks, forming the side walls of the fissure. These cracks thus filled are termed veins—*mineral veins.* To account for the filling of these veins by minerals, many speculations and

theories have been advanced. Some suppose that the minerals, which are often combinations of the oxides and sulphurets of metals, have been injected in a fused state from below. Thus M. Agassiz, speaking of the beds of copper found at Lake Superior, says,—"It must have been of pre-historic origin that the copper has been thrown up in a melted state, as it were boiled up. In places where great quantities have come through, and the rock very compact, it has remained unaltered by other influences; but where it was thrown up in less quantity, and the rocks not so compact, it has been oxidised and combined with other compounds, as carbonic acid and sulphur, &c. Nearest the metallic beds the copper is found in the state of oxide; then as we proceed farther, it is found as carbonate and sulphuret, and coming to a greater distance, it is all sulphurets."

Another theory ascribes the filling of these veins to emanations of mineral matters in the form of vapours, also from the centre, analogous to the vapours given off from volcanoes, this hypothesis being supported by the fact that metals having analogous properties of sublimating, are often found together. A third, that water holding minerals in solution has passed into the crack, or fissure, and the water having evaporated, has left the mineral; or that the minerals have been crystallized from their solution, by a galvanic, or electric current.

It is not my intention to discuss these theories in the present paper. It is sufficient to say, that there is much to be said both in favour, and against each, and many practical difficulties stand in the way of their adoption. There is, probably, no subject connected with the physical sciences, where the theories of the scientific observer, and the opinions of practical men are so universally opposed, as upon the origin of the filling of mineral veins. And although the practical miner may not be the best authority upon the causes which are, or have been, at work, to fill the veins which he may be engaged in emptying; nevertheless, we have always found that the observations of intelligent working-men are worthy of some attention, and should not be thrown overboard in order to clear the way for the establishing of some favourite theory, until they have been subjected to a rigid scrutiny, and proved to be useless.

An opinion prevails amongst miners, that minerals grow, from observing them effloresing, or crystallizing, from the sides of the rocks; and, also, that certain veins have been known to become richer in metal, in the course of years, either by the metal increasing, or the matrix decreasing. I will here transcribe the opinion of a practical mining engineer, Mr. Evan Hopkins, who supposes that not only the veins have been filled, but that the crack or fissure, in the rock, is the result of a magnetic action.

"Numerous instances may be mentioned, where old workings have been partially filled with a fresh crop of minerals; and also where minerals have been decomposed, and disappeared.

These chemical actions, governed by the subterranean polar currents,

continue to fill every fissure or vacuity with crystals, the growth of which swells open the crack, and thus causes new fractures and dislocations, according to the variable nature of the containing rock, and the amount of resistance. This gradual opening of the veins, with the growth of the crystals from the sides, accounts for the isolated masses of the bounding rocks found in veins, which could not possibly occur, had they been open fractures. Indeed, the hypotheses which supposes minerals to be *filled by solution from above;* or that of the *injections of igneous matter into an open fissure from below,* are so crude and irreconcilable with the contents, that they do not deserve our attention. The facts brought forward fully justify the conclusion, that all veins, whether they be mineral or not, have been formed and filled upon the same principle of polar action, as described. In the east and west, or transverse fissures, the crystals are formed from side to side; and in the splits, longitudinally, in parallel plates," &c.

Such, then, are, briefly, a few of the conflicting ideas upon the filling up of mineral veins, exhibiting the humiliating fact, that, as yet, we know nothing about it, and that many observations and experiments must yet be made, before all the conditions and facts known will harmonize. We cannot penetrate to the depth of a vein, to trace the orifice through which the fluid mineral may have flown, or their gaseous emanations may have filtered. The beautiful crystalline appearance of some of these minerals, however, form no objection to the igneous theory, as some of the samples of slags exhibited will show, although we have heard such objections made. In examining some of the crystalline minerals, it is evident, in many instances, that the crystals have been formed under the influence of a current of some sort, both constant and of considerable power, as the specimens on the table will show. Where and when the objects upon which the crystals have formed protrude, the crystals are only upon one side of the protruding object, and in one direction, such as is often seen in cabinet specimens, where sulphuret of iron, carbonate of iron, fluor and calcareous spar, &c., &c., have crystallized upon other crystals, such as quartz, that these minerals have formed only on one face of the quartz crystal; or, if the edge of the crystal has been exposed to the current, two faces are covered with the mineral, none being behind. So that if the stone, or specimen, be held in a certain position, the line and direction of the current may be easily traced. And we find this crystallization, or deposition, has taken place with minerals, and upon minerals, so that the idea of their having been in a fused or vapourous state by heat is inadmissible; at the same time, the crystalline mineral must have been either in a gaseous, or fluid state, and that for a considerable time, to be thus formed, and subject to a powerful influence, which produced and maintained the crystallization upon one face of a crystal, or series of crystals, and not over the whole surface. Whether this force be polarity in the crystals themselves, or a general polarity over the mass, we are not in a position to assert.

As the formation of many of these minerals may depend upon causes constantly in operation, I have thought it possible that indications of these might be obtained in detached stones, found in the alluvial deposits. My search into these have been, as yet, limited, but by drawing the attention of the members of the Society to the subject, more extensive observations may be made, and either the utility, or futility, of the research manifested.

In some of the loose stones found in the alluvial bed, are seen changes that have evidently taken place since they were detached from the rock, and analogous to those changes that are taking place in the rock.

The first changes I refer to are well known; it is, that water in contact with, and passing through, rocks, changes the character of these, by dissolving out some of their component minerals. A piece of trap rock, for example, exposed to water, very soon changes, when alternately wet and dry, and exposed to the atmosphere; the decomposition is sensibly apparent, a brown crust is soon formed, which becomes soft and brittle, breaking off by slight friction, leaving a new portion of the stone to undergo the same change. The same sort of stone imbedded in the gravel under the soil, passes through the same changes, but the crust, in this case, is not so soft and brittle; the change soon penetrates to the centre of the stone, giving it a different character and appearance. Analysis of the stone so changed, compared with the original, makes the change very apparent. We give the average of many analyses, from different localities:—

Kernel, or Original Stone.		Crust, or Altered Stone.	
Insoluble Silicate of Alumina,	66·8	Insoluble matter,	72·5
Protoxide of Iron,	18·5—Fe 14·4	Peroxide of Iron,	19.7—Fe 13·8
Lime,	3·8	Lime,	0·9
Magnesia,	1·5	Magnesia,	0·3
Potash,	2·6	Potash,	trace
Loss at red heat water,	6·2	Loss at red heat,	5·8
	99·4		99·2

Here, then, we find that water has dissolved out, lime, iron, magnesia, and potash, and the remaining iron changed to the peroxide state. The length of time required for the water to penetrate a piece of trap I know not, but that the soluble power of the water is great, is evident, by placing a piece of such rock in distilled water; 100 grains digested for 6 days, at summer heat, lost one per cent., the water had an alkaline reaction, and contained magnesia, potash, and lime. Now, if a crack or fissure existed in such a rock, and water oozed through, which it does, more or less, in all rocks, the crack would unavoidably get filled with lime in a state less or more crystalline, according to circumstances, lime being much less soluble in water, while the potash, and probably mag-

nesia, might pass off with the remaining water. Many whinstones contain a great quantity of lime as a component, which makes this reaction more certain; and veins of calc spar are abundant in the trap rock—a specimen taken within a few inches from a vein of spar, contained 9 per cent. of lime.

The change by water is not confined to the trap rock. We have here a boulder of bastard limestone, completely changed in its character for several inches deep, so as to give no idea of its original appearance by external observation. The following is the nature of the change:—

Kernel.		Altered Part.	
Silica	12·2	Silica	28·4
Carbonate of lime	22·5	Carbonate of lime	3·6
Carbonate of iron	42·8	Peroxide iron	40·5
Carbonate of magnesia	12·4	Carbonate of magnesia	1·4
Sulphur	1·1	Sulphur	3·5
Water	8·4	Water	21·3
	99·5		98·7

Thus the same class of change has taken place, as with the trap, lime and magnesia has dissolved, and the iron, peroxidised, has lost its carbonic acid. We have occasionally found stones in the gravel deposit that apparently have been broken in two, and the two halves lying very close to each other, have received a deposition of minerals between them, so as occasionally to cement the two parts together at the points closest to each other, such as in the sample present—this sample stone before you, which is composed of

Insoluble matter	60·5
Protoxide of iron	22·5
Carbonate of lime	6·6
Magnesia	0·8
Sulphur	2·5
Loss at red heat	6·6
	99·5

The three minerals filling up this fracture, namely, sulphur, iron, and carbonate of lime, are also, we observe, component parts of this stone, but we would not venture to say that the stone is the source of these minerals, neither would we assert that the deposition of these minerals has taken place in the alluvial deposit in the position in which the stone was found,—the question suggesting itself being, why the crystallization only goes on in the fracture, and not upon any other part of the stone. Should these mineral depositions be found to take place in such stones in their position as boulders, it will show, what we believe to be the case, that there is some influence effecting mineral depositions in veins or fractures, or even in the fragment of a rock, when these are placed in

proper circumstances. Another boulder of a singular sort, exhibiting the powerful action constantly going on in the earth to change the character of rocks, is the specimen now shown, the crust of which is of the appearance and composition of trap rock, while the kernel is black, and more like a piece of shale. The crust or outer portion of stones of this sort have generally been looked upon as incrustations, from the water in which they have been placed having salts in solution, that have become deposited upon the stone or kernel portion. Such incrustation does take place upon organic substances placed in mineral waters; but in this instance, as in most other minerals, the change has evidently been caused by the decomposition of the original stone. The analysis of the crust and kernel will show the change that has taken place in this instance.

	Original Stone.	Crust.
Insoluble	76·0	64·7
Protoxide of iron	11·2	16·3
Carbonate of lime	1·4	7·5
Magnesia	1·1	3·3
Sulphur	3·3	3·2
Coaly matter	5·7	0·4
Water	1·5	3·7
	100·2	99·6

Here we find the coaly matter disappearing, no doubt becoming oxidised, and in all probability fixing from the water a quantity of lime as carbonate.

Another class of boulders gives a still more decided specific character, and exhibits the same character of phenomena as some mineral veins, and these changes evidently have taken place in the boulder since they were detached from their parent rock. One or two specimens will illustrate these remarks. The first specimen is a dark crystallized carbonate of iron, and has been acted upon from the surface, a vein of iron pyrites marking the line of division between the crust, or part acted upon, and the kernel, or portion not decomposed.

Kernel.		Crust.	
Silica	6·5	Silica	37·2
Protoxide of iron	60·3	Protoxide of iron	30·0
Carbonic acid	32·7	Sulphuret of iron	9·6
		Copper pyrites	0·8
	99·5	Carbonic acid	21·7
			99·3

The changes here are very evident, and that they have taken place since the stone became a boulder, we have only to trace the form and position of the changed portion and vein of pyrites, and also that the change has been caused by water penetrating the mass which has contained

sulphur and copper. The origin of the iron pyrites vein, so regular and distinct, is worthy of notice, and not easily accounted for, but by a sort of polar action between the crust and kernel causing a line of demarcation at the junction, marking the central point of the influence.

Such veins of *mundic*, sulphuret of iron, in rocks, are generally ascribed to heat, and more especially to sublimation, as we referred to at the commencement of this paper; but under the circumstances in which such boulders as this have been placed during the formation of this vein, no such cause as heat or sublimation can be admissible. Both animal and vegetable substances imbedded in the earth, and subject to the action of water containing sulphate of iron, undergo decompositions; the sulphuric acid is decomposed, and sulphuret of iron formed. Thus many fossils are converted into pure pyrites, but these conditions differ from this, although we have heard these also ascribed to sublimation.

We have met with many other specimens of carbonate of iron having veins in them of iron pyrites, but in all cases the composition of the mineral internally to the vein of pyrites, represented by the kernel, was of a different composition to that part externally to the vein represented by the crust, as in this second specimen, in which the analysis exhibits the same character of changes as were shown in the trap rock—thus:

Internal to pyrites vein.		External to pyrites vein.	
Silica	6·4	Silica	23·2
Protoxide of iron	48·6	Protoxide of iron	37·4
Iron pyrites	5·4	Iron pyrites	15·0
Lime	3·2	Lime	1·0
Magnesia	2·0	Magnesia	0·4
Carbonic acid	32·7	Carbonic acid	22·7
	98·3		99·7

We will refer to one other kind of boulder, or nodule, of very common occurrence, formed all of iron pyrites, similar to the pyrites fossils referred to. Nodules of this sort are of frequent occurrence in chalk and clay, many of them having, no doubt, an organic origin. The centre of these nodules, although they have always a different structure from the crystalline crust, differ little in composition, but when such nodules are found in mineral veins, where several other metals exist, as in this specimen, which is from the Parys mines, Anglesea. The centre and crystalline crust are very distinct in composition, as shown in the following analysis:—

Crystalline crust.		Kernel.	
Silica	1·2	Silica	23·2
Pyrites — Iron	45·6	Iron pyrites	55·2
Pyrites — Sulphur	52·4	Galena	5·6
		Zinc blend	14·5
		Sulphate of lime	1·1
	99·2		99·6

Here we have an opposite result from the changes produced by water, the kernel being the most impure. The nature of the surrounding matter to such nodules would require to be also considered, when we might see the course of a similar polarization between it and the centre, inducing the formation of that pure line of crystalline pyrites crust, which separates the centre from the external rock or mineral, in the same manner as in the former specimen.

We have thus, in a somewhat desultory manner, recorded the results of observations made during some investigations for another purpose. The changes which are taking place in these loose stones are, we believe, taking place in the rocks forming the crust of the earth, and if these changes be found analogous, observations may be more easily extended, being more within every individual's reach.

We must not be supposed in the meantime as either objecting to or supporting any particular theory of the filling of mineral veins. If they have been filled by the minerals, whether metallic or otherwise, being dissolved out of the rock, and carried in solution to the fissures, where they become reduced, either by ordinary crystallization or electrical influence, they must have been diffused through the rocks originally. The solvent that would dissolve out all the metals that are found in veins from such rocks in which the vein exists, without dissolving in the first instance many of the principal earthy minerals, even although accompanied with strong magnetic currents, it would be difficult to suggest.

There are, again, so many strong and practical objections to the igneous and sublimation theories, in almost every step we take, that we think it best to pause until a more rigid chemical investigation has been made of substances under every condition in which they may be found, as the practice of theorizing upon external observation is as likely to lead to error as to truth.

March 17, 1852.—*Mr.* CRUM *in the Chair.*

THE following communication was made:—

XXIX.—*Notice of the Vinegar Plant.* By Dr. R. D. THOMSON.

THE Vinegar Plant, or mother of vinegar, belongs, according to Kützing, to the genus *Ulvina*, characterized by consisting of a compact lubricous layer of very minute granules. *Ulvina aceti*, at first membranaceous, then forming a compact stratum, vertically divided into dichotomous branches densely aggregated. He describes it as occurring always in the vinegar fermentation, upon the *surface* of the vinegar pot. The formation of the vinegar plant commences with that of the vinegar. It begins as a thin pellicle on the surface of the fluid, with little consistence. Under the microscope it consists of small globules, which are six

times smaller than those of yeast. This pellicle becomes thicker, more compact, and coherent, and in fourteen days it begins to grow on the exterior border. It then presents the aspect of a *chætophora*—a gelatinous, fucoid appearance.

To endeavour to throw some light upon the mode of action of the vinegar plant, I inserted a portion of a plant, well washed with distilled water, in a solution of sugar, and exposed the whole to the influence of the air. The liquid, when first formed, had no action on litmus paper, but in a few days it was characterized by a distinct acid reaction. After some weeks I took a portion of the fluid, saturated it with carbonate of soda, and distilled it in a glass retort. A liquid passed over which possessed the odour of alcohol, and which gave aldehyde and green oxide of chromium when treated with bichromate of potash and sulphuric acid, according to a mode of testing which I described some years ago. (Proc. Phil. Soc. Glas., ii. 94.) After two-thirds of the fluid in the retort was distilled, the receiver was changed, and sulphuric acid was poured into the retort, heat being applied cautiously. An acid liquid passed over into the receiver, which possessed the odour of vinegar, and rendered yellow a colourless solution of sesquichloride of iron. It was therefore acetic acid. Another experiment was made to determine the nature of the products of the vinegar plant, in the absence of oxygen. An ounce of sugar was dissolved in twenty ounces of water, a vinegar plant was introduced into the solution, the bottle was stopped close with a ground stopper, carefully waxed, and inverted in a glass full of distilled water. After some weeks, only a small portion of fluid was found in the bottle, smelling strongly of alcohol, and yielding aldehyd and green oxide of chromium to bichromate of potash and sulphuric acid. The stopper was still fixed in the bottle, but the wax had given way in one place by the pressure of the gas, so as to allow of the expulsion of the fluid into the exterior vessel filled with water. The greater portion of the ⅞ bottle was filled with a gas, which upon examination was found to precipitate lime water abundantly, and to be absorbed by caustic soda. It was, therefore, carbonic acid.

Formation of Alcohol.—From these experiments it seems undoubted that the vinegar plant possesses the power of breaking up sugar in its solutions into alcohol and carbonic acid, and as the plant during the process appears to be increasing in bulk, it seems scarcely legitimate to ascribe the decomposition of the sugar to any process of decay in the plant itself. I am not prepared from my own observation to state that, in absence of air, the vinegar plant when immersed in a solution of sugar, does increase in bulk, or, in other words, grow, although I have no reason to doubt the fact. But from the observations of Schlossberger and Schmidt, there can be little hesitation in concluding that the vinegar plant is merely a modification of the yeast globules, and therefore that it is capable of augmenting in size under the same conditions as the latter form of vegetation. When a plant of this description is found to

vegetate in a position of immersion in a liquid, it must possess a power of extracting nourishment from the fluid atmosphere with which it is surrounded, just as sea and water plants effect their object. It is, however, impossible that in a solution of pure sugar the vinegar plant can increase from the influence of vinegar and any albuminous substance, as has been asserted (Mulder Scheid. Onderz Deel. i. 539,) to be the mode of its propagation, since the presence of the vinegar plant precedes the formation of the vinegar in the trials detailed. If the numerous experiments of various chemists are to be depended on, it is certain that the cellular structure of the vinegar plant, consisting of cellulose chiefly, must derive its carbon from the carbonic acid of the sugar, in absence of common air, or possibly from the atmosphere, in its presence, which may also supply it with nitrogen for its albuminous constituent; or the nitrogenous principle, like the salts, may be capable of a greater diffusion, without any considerable increase in its original amount.

Formation of Vinegar.—The circumstances most favourable to the production of vinegar from sugar are, when a vinegar plant is introduced into an open shallow vessel, containing a solution of sugar or treacle. The plant exposes its upper layer near the surface of the solution, and augments by the deposition of a new layer or stratum above the old plant, to which it is attached, but both can readily be separated simply by lifting up the superincumbent layer. It is thus worthy of notice, that the new growth takes place between the old plant and the atmosphere that is in closer contact with the air. My observations tend to show that when the vinegar plant falls to the bottom of a deep vessel filled with a saccharine fluid, the progress of the acetification is much more slow than when the plant is in contact with the air. The action of a cellular plant is, under these circumstances, analogous to that of a porous body which is capable of condensing oxygen from the atmosphere, to a condition approximating to fluidity upon the area of its wall cells. The action of spongy platinum and platinum black in the absorption and condensation of oxygen, are sufficiently well known; and through this power, of oxidizing and acidifying, hydrous oxide of methyle into formic acid, and alcohol into vinegar. The absorption and retention of air in the cells of the vinegar plant may assist in explaining the fact, which is particularly noticed by Kützing and Schmidt, that it is distinguished by its floating at the surface of the fermenting fluid, while the yeast globules remain at the bottom of the liquid.

The influence of cellular plants in decomposing the higher oxides, in consequence of their absorptive action on oxygen, is well exemplified in the case of yeast globules, which, when brought in contact with binoxide of hydrogen, speedily cause the removal of the second atom of oxygen. (Liebig Ann. v. 211.) This action seems quite parallel to that of porous paper on certain coloured solutions, as the red solution of permanganate of potash, and the amethyst solution of ferrate of potash, the former of which is slowly, the latter with great rapidity, deprived of tint when

passed through common filtering paper. The yeast globules and vinegar plant, when introduced into a solution of permanganate of potash, remove its fine colour rapidly, but the action is not so instantaneous as when these globules are placed in the ferrate of potash. The action in these instances appears to be parallel to that of the deoxidation and decolourization of the permanganate of potash by spongy platinum, which is, however, somewhat more slow than that of the influence exerted by the yeast plant. It is in this way also that the cellular matter, in the form of chips of birch, acts in the formation of vinegar from alcohol in the quick vinegar process.

ANALYSIS OF THE VINEGAR PLANT.

107·05 grs. gave 101·2 water.
— 6·85 solid residue.

The constitution of 100 per cent. was found.

Water	94·53
Cellulose	5·134
Alkaline salts	·336
	100·

The salts, when dissolved in water, indicated the presence of chloride, sulphate, and a trace of phosphate. The plant, when digested in weak caustic soda, gave a turbidity when the alkaline fluid was saturated with acetic acid, indicating the presence of some albuminous matter.

Mr. Alexander Harvey exhibited and described Mr. Kennedy's water meter.

March 31, 1852.—*Mr.* CRUM *in the Chair.*

The Council reported, that after carefully considering Mr. Robert Blackie's motion for a change in the method of electing members of Council, they agreed to recommend its adoption by the Society, with this modification, that the entire Council should retire from office every year; eight of the twelve members being re-eligible, and four not being re-eligible until they shall have been out of office for one year—the election to be regulated otherwise in the manner proposed to be enacted by the motion.

Mr. Blackie having expressed his concurrence in the alteration of his motion, the Society unanimously approved of the Council's report; and it was further resolved, by the first vote of the Society, that the substance of the following regulation respecting the annual election of office-bearers, shall be embodied in the thirteenth section of the constitution of the Society, in place of the penultimate clause as at present, namely:—

"The president, the treasurer, the secretaries, and the librarian, may be re-elected. Neither of the two vice-presidents shall be eligible for

more than two years successively. Of the twelve retiring members of Council, four shall not be re-eligible till they have been out of office for one year. The names of the councillors to be arranged in accordance with the number of votes recorded for each, the one having the greatest number of votes to be placed at the bottom of the list, and the others progressively above. The four councillors at the top of the list to be those who retire, and are ineligible till they have been out of office for one year."

The Council also reported that they had taken into consideration Mr. Bryce's motion, that the Society shall celebrate its jubilee year at the beginning of next session by a public dinner, to which several eminent scientific men might be invited. The Council recommended the proposal to the adoption of the Society, suggesting that it be remitted to the Council to make the necessary arrangements, and to constitute a committee of its number for this purpose, together with the following additional members from the Society, viz.: The Lord Provost, Sir James Anderson, James Campbell, Esq., younger of Stracathro, William Brown, Esq., William Bankier, Esq., Dr. Strang—the committee to have power to add to their number. This was agreed to.

Professor Allen Thomson described recent discoveries in regard to the Reproduction of Invertebrate Animals.

April 14, 1852.—DR. WALKER ARNOTT *in the Chair.*

A SECOND vote was taken on the proposed alteration of the rule for the election of members of Council, which was finally agreed to.

Mr. R. M. Murray read a paper on "The Water of the Dead Sea."

Mr. Paul Cameron read a paper on "The Force of Vapour from Saline Water, as applied to Marine Engines."

XXX.—*Examination of the Waters of the Dead Sea.* By ROBERT M. MURRAY, Esq.

THE Dead Sea, as is well known to every person acquainted with geography, is situated in the south-east of Palestine, at a distance of about fifteen miles from Jerusalem. It is mentioned in Scripture under the several appellations of the "Salt Sea," the "Sea of the Plains," and the "East Sea." It is the "Lacus Asphaltites," or Bituminous Lake of the ancients, and the "Bahr Lout," or Sea of Lot of the Arabs. It lies in a deep caldron, surrounded by high cliffs of bare limestone rock,—the western range being 1500 feet above the water, and the eastern range about 2500. Its breadth is about 9 miles, and its length 39 or 40. According to the survey of Lieut. Lynch in 1847, its depth varies from 114 to 218 fathoms in the north end, and from 2 to 18 in the south end.

One of the most singular circumstances in the character of the Dead



Sea, is the great depression of its level below that of the Mediterranean. According to Bertou, the difference of level is between 1300 and 1400 feet. The Dead Sea has no outlet, but it is now the received opinion that it loses its waters by evaporation, which, except in the rainy season, is sufficiently great to counterbalance the influx from the Jordan. On the surface of the Sea there is often found floating an immense quantity of asphaltum, which is collected by the Arabs and sold for medicinal and other purposes. Sulphur is found on various parts of the shore, which is also collected by the Arabs, and used by them for making their gunpowder. Small lumps of nitre and pumice-stone are found occasionally. The specific gravity of the water is so great, that it is almost impossible for a man to sink in it. Lieut. Lynch was overtaken by a storm on the Lake, and he states that, from the density of the water, it seemed as if the boats were encountering the sledge-hammers of the Titans, instead of the opposing waves of an angry sea; and that when the wind abated, the sea as rapidly fell,—the water, from its ponderous quality, settling as soon as the agitating cause had ceased. Bathers in the Lake experience a curious sensation of the eyes—a kind of temporary blindness, and upon getting out of the water, the evaporation leaves a thick oily incrustation of salt on the skin, which remains for many days, as it is impossible to remove it completely, even by repeated ablution.

Lieut. Lynch states that on dredging the sea at some places, cubic crystals of salt were brought up along with the mud. There are also several mines of rock salt in the sides of the mountains on the western coast; indeed Usdum, a mountain in the south-west extremity of the Lake, is a solid mass of rock salt.

The water has a slightly greenish hue, and is not entirely transparent, but objects seen through it appear as if seen through oil. Its taste is intolerably nauseous and bitter. The first analysis made of the water was by Dr. Perry in 1742; but from the experiments he made, he could not conclude whether the water was impregnated with anything besides common salt and something of a compound nature, partly sulphureous and partly bituminous. It was analysed by Lavoisier in 1778; by Dr. Marcet of London in 1807, who operated upon a small quantity of the water; by Haproth in 1809; by Gay Lussac in 1818; by Professor Gmelin in 1826, who first discovered the presence of bromine in it; by Dr. Apjohn in 1839,—his specimen was taken at half-a-mile from the mouth of the Jordan, near the close of the rainy season, which may account for its lower specific gravity. It has also been analysed very recently by Messrs. Herapath of Bristol, and by MM. Boutron Charlard and O. Henry, in March last. The specimen analysed by the latter chemists was obtained at a distance of two hours' march from the Jordan, on the 2d April, 1850, one of the months of the rainy season. It exhibited a lower specific gravity than any specimen previously analysed, being only 1·099, and the per centage of salts was only 14·93. In all the analyses which have been made of the Dead Sea water, the total amount

of salts is found to be nearly the same, but the relative proportions of the different salts vary greatly. These varying results are partly accounted for, by the specimens of water analysed having been collected at different seasons, and at different parts of the Lake,—their composition thus being modified by the proximity of the Jordan and other streams. During the rainy season, the Lake rises ten or twelve feet. A sheet of fresh water of that depth will thus be thrown over the Lake, which water may be supposed to flow over a fluid nearly in 1·2 in density, without greatly disturbing it. The salts rise from below into the superior stratum of fresh water by the process of diffusion, which will bring up salts of the alkalies with more rapidity than salts of the earths, and chlorides of either class more rapidly than sulphates. The composition of the water near the surface must therefore vary greatly as this process is more or less advanced.

The specimen which I have analysed was collected in April, 1847, on the western shore of the Lake, and about a mile from the mouth of the River Jordan. Its specific gravity at 60° Fah. was 1·156. Its taste was intensely bitter and nauseous, but it had no unpleasant odour. There was no definite reaction with either blue or reddened litmus paper, proving the absence of any free acid or carbonated alkali. It did not affect acetate-of-lead paper, proving the absence of sulphuretted hydrogen. Qualitative testing showed that it contained lime, magnesia, alumina, potass, soda, chlorine, bromine, and sulphuric acid. Not a trace of iodine could be detected. On the sides of the bottle in which it had been kept, a deposit was formed, which was found to consist of carbonate of lime, peroxide of iron, organic matter, and silica.

Quantitative Analysis.

100 grs. evaporated to dryness with 10 grs. of pure carb. soda, gave as the per centage of saline ingredients, 22·09.

1000 grs. of the water were precipitated by BaO, NO^5, and the precipitated BaO, SO^3, weighed 1·51 grs. = ·0517 of sulphuric acid per cent.

500 grs. were precipitated by $NH^3 + NH^4Cl$, which gave ·05 of alumina = ·01 per cent.

The solution filtered from the alumina was precipitated by NH^4O, $O + NH^4Cl$, which gave 10·50 grs. of carb. lime = 1·17 per cent. of lime.

The solution filtered from the lime was precipitated by phosphate of ammonia, which gave 4·48 per cent. magnesia.

500 grs.—The alumina having been separated by NH^3, the bromine was precipitated by a saturated solution of Ag Cl in NH^3, which gave 2·37 of Ag Br = ·20 per cent. of bromine.

50 grs were precipitated by AgO, NO^5, which gave 57·96 grs. per cent. of mixed Ag Cl and Ag Br. Deducting ·47, the per centage of Ag Br, gives 57·49 of Ag Cl = 14·23 per cent of chlorine.

100 grs. gave of mixed chlorides of sodium and potassium, 9 98. The

potassium was separated by Pt Cl^2, which gave 7·64 grs. of the K Cl+ Pt Cl^2 = 1·47 per cent. of potass, leaving 4·044 per cent. of soda.

100 parts of the Dead Sea water yielded accordingly—

Sulphuric acid	·051
Chlorine	14·230
Bromine	·200
Alumina	·010
Lime	1·170
Magnesia	4·484
Potash	1·470
Soda	4·044

Consequently its true composition will be—

Sulphate of lime	·086
Carbonate of lime	traces.
Chloride of calcium	2·245
Chloride of aluminum	·024
Chloride of potassium	2·330
Chloride of sodium	7·650
Chloride of magnesium	9·560
Bromide of magnesium	·231
Peroxide of iron	traces.
Silica and organic matter	traces.
	22·126
Total amt. of salts by actual experimt.	22·09

Composition per Gallon.

Sulphate of lime	69.59
Chloride of calcium	1·816·66
Bromide of magnesium	186·92
Chloride of magnesium	7·737·95
Chloride of aluminum	19·42
Chloride of potassium	1·885·43
Chloride of sodium	6·190·38
Carbonate of lime	traces.
Peroxide of iron	traces.
Silica and organic matter	traces.
Fixed salts	17·906·35
Water	63·014·20
Weight of a gallon	80·920·55

XXXI.—*The force of Vapour from Saline Water, as applied to Marine Engines.* By PAUL CAMERON.

WHEN the following experiments were made, the barometer stood at 29·6, the temperature of the room being 62°.

I exposed a quantity of salt for three hours, and placed a thermometer in it, until it indicated a temperature of 60°. A glass globe was procured, two inches in diameter, with a stem ten inches long; to the stem was attached a scale, divided from 60° to 233°, representing the expansion of pure and saturated water; the glass globe and stem were filled with pure water at 60°, the thermometer indicating the same heat. The glass globe was then placed in a tin vessel, containing water, on the fire; the boiling point was marked on the scale attached to the stem, which is divided so as to represent the scale of a thermometer: from this I was enabled to determine the expansion of pure water, and to compare it with water saturated with salt.

The globe was again placed in the tin vessel over the fire till the water boiled; it was then quickly immersed in a tin vessel containing four gills of water, at 60°; in five minutes the water in the tin vessel rose to 82° 5′, and in forty minutes it fell to 78°. The glass globe, or water thermometer, was filled with saturated water, and placed in the tin vessel containing boiling water, and remained there till the saturated water in the stem remained stationary at 220°; it was then quickly immersed in the tin vessel, containing water at 60°, and in five minutes the water in the tin vessel rose to 82°, and in forty minutes it fell to 78°. The water in the tin vessel was then saturated till its boiling point was 226°. The water thermometer was again placed in the tin vessel, and stood at the boiling point, when its expansion was 233°; it was then quickly immersed in water at 60°, and in five minutes the water rose to 86°, in forty minutes it fell to 81°, and in one hour it fell to 78°.

A tin vessel was procured, 3½ inches diameter, 8 inches deep, containing four gills of water, with salt in solution: its boiling point previously known to be 216°. Two thermometers were made to pass through a stuffing box in the cover, the scales being divided on the right hand side to indicate the temperature, and on the left to indicate the pressure in pounds on the square inch; the bulb of one being immersed in the water, the other placed in the space for the steam. On the top of the cover were screwed two stop-cocks; attached to one of them was a mercurial gauge to indicate the pressure of the steam, the other used for blowing off when required. The spirit lamp being then applied, and the water made to boil, the thermometer, immersed in the water, indicated 216°, and the one in the steam indicated 215°. At this stage of the experiment, the mercurial guage began to move along with the thermometer; the steam was then allowed to blow off, and the water removed.

An equal quantity of water with salt in solution, its boiling point being 221° 8′, was then placed in the tin vessel over the spirit lamp; when the

water boiled, the thermometer in the water indicated 221° 8′, and the one in the steam indicated 220° 5′: at this point the mercurial gauge indicated a rising along with the thermometer.

The water was then removed, and replaced by saturated water, its boiling point being 226°, when similar results followed as in the former case. The apparent difference of heat in the steam and water, I think, arises from the steam being more sensitive to cooling effects.

It will be evident, from these experiments, that the water and steam are nearly of the same temperature, and whatever the amount of saturation may be, its vapour can only balance the atmosphere when the water is at the boiling point.

This brings me to the point that has been called in question by many, as they maintained that the vapour from saline water, at 212°, was equal to the pressure of the atmosphere.

From the above experiments, and the experiments which follow, I hope you will be satisfied that such is not the case.

This led me to examine the vapour from saline water, to ascertain whether it did not contain a portion of salt.

To the blow-off stop-cock was attached a glass tube, having a globe at its end, which was immersed in water. The vapour from saturated water was then allowed to pass freely into the globe to be condensed. I then removed it, and applied a few drops of nitrate of silver, when, at the moment of contact, it showed the condensed vapour to contain salt. This may be one of the causes why vapour, from saline water, requires a greater amount of heat, compared to that from vapour from pure water, as the salt contained in the vapour must occupy a certain amount of its bulk, therefore its elastic force must be less in proportion to the extent of saturation. It was from these deductions I was led to the construction of one of the scales on the salinometer, showing the decrease of the elastic force in proportion to the amount of saturation.

From deductions made from a few of the foregoing experiments, I was led to the construction of the two other scales on the salinometer.

In a former paper I stated it was necessary, that in proportion to the extent of saturation, so must there be a proportionate increase of injection water to the condenser, for the engine to work effectively. It is evident (from tables which I have drawn up) that, in proportion to the extent of saturation, there is a proportionate increase of heat and loss of elastic force; hence the necessity of care being taken to keep the water as free as possible from increased saturation. On a previous evening I did not refer to any other instrument used for finding the quantity of salt contained in the water of marine boilers; but I beg to take the liberty now of referring to them. The scales of the different instruments I have seen used for this purpose, have been constructed from experiments made with salt dissolved in cold water, in different proportions, so that the one in present use cannot give even an approximation, as the saturated point of boiling water will be upwards of three pounds to the gallon, and that of

cold water about two pounds. This being the case, it may be a correct instrument as applied to cold water, but never can be correct as applied to the water of marine boilers.

April 23, 1852.—Mr. Crum *in the Chair.*

The concluding meeting of the session was held this evening.

It was reported from the Council that it had agreed to meet on the 4th of August, for the purpose of appointing delegates to represent the Society at the meeting of the British Association at Belfast.

The Council proposed that the Jubilee Dinner of the Society shall take place on the 9th of November, when the Society completes its fiftieth year. It was recommended that members be allowed to introduce their friends on the occasion. The arrangements were left in the hands of the Council.

The following papers were read:—

1. Mr. J. A. Campbell—"Visit to the Quicksilver Mine of Idria."

2. Mr. C. J. Hughes—"Remarks on Binocular Vision and the Stereoscope, with Photographic Illustrations."

3. Mr. Harvey exhibited and described Bourdon's Steam Pressure Guage.

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PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

FIFTY-FIRST SESSION.

November 3, 1852.

THE Fifty-first Session of the Philosophical Society of Glasgow was opened this evening. Mr. Walter Crum, the Senior Vice-President, in the chair.

The following minutes of Council were read, of date October 26:—

"On the motion of Mr. Liddell, it was resolved unanimously, that before proceeding to business, the Council should record in the minutes an expression of their sorrow for the death of their late distinguished and venerable President, Dr. Thomas Thomson; and of the estimation in which he was held by them for his eminent scientific attainments and discoveries; for his untiring attention to the business of the Society, the meetings of which he regularly attended, till a comparatively recent period, during the eighteen years in which he occupied the chair; and also for his other valuable qualities. The Council at the same time resolved to express their deep sympathy with the family of Dr. Thomson in their bereavement. Further, that Mr. Crum, the Senior Vice-President, be requested to prepare for the first meeting of the ensuing Session of the Society a sketch of the life and labours of Dr. Thomson.

"It was also agreed, on the motion of Mr. Liddell, to record the thanks of the Council to Dr. Robert Thomson, on his leaving this city, to settle in London, for his valuable services as Librarian to the Society, in which capacity he has introduced various important improvements, more especially by his painstaking efforts to complete several sets of previously imperfect periodicals, and to obtain copies of rare and valuable works; and also for his editorial superintendence of the Society's printed "Proceedings." In acknowledgment of these and other services to the Society, rendered by Dr. Robert D. Thomson, the Council resolve to recommend to the Society that he be elected an Honorary Member.

"The Council unanimously and cordially agreed to recommend to the Society that Mr. Crum be elected President; and that Dr. Allen Thomson be elected to the vacant chair of Vice-President.

"It was also resolved to propose to the Society that the law of last

Session limiting the tenure of office of the Vice-Presidents to two years in succession, be now also made applicable to the President, so that he shall not be eligible for more than two years consecutively.

"The Council agreed to recommend to the Society that Mr. William Cockey be appointed Librarian."

Mr. Liddell reported on behalf of the deputation from this Society to the British Association, that that body had given the preference to Hull for its meeting of next year, but that there was reason to hope that Glasgow would be honoured with a visit from the Association in the year following.

The Society, by acclamation, elected Dr. Robert D. Thomson an Honorary Member.

Dr. Thomson expressed his sense of the honour conferred upon him, and stated that his connection with this Society had always been of the most delightful kind; and although he was about to remove to a considerable distance, he still hoped to have the pleasure of occasionally visiting the Society, the continued success of which would afford him sincere satisfaction.

The first vote of the Society was taken on the recommendation of the Council, that the law with respect to the Vice-President should in future be made applicable to the office of President. The proposal was affirmed.

Mr. Clark, Curator of the Royal Botanic Gardens, placed on the table a variety of exotic plants in flower.

Mr. Smith of Sheffield, who was introduced by Mr. Cockey and Mr. Liddell, exhibited and explained a heating apparatus.

The Vice-President, Mr. Crum, then read a Sketch of the Life and Labours of the late Dr. Thomas Thomson, President of the Society:—

XXXII.—*Sketch of the Life and Labours of Dr. Thomas Thomson, F.R.S. President of the Philosophical Society.* By Walter Crum, F.R.S.

Dr. Thomas Thomson was elected President of this Society on the 12th of November, 1834. During the eighteen years which have since elapsed, his attention to the duties of the chair was unremitting, until the beginning of the session in November 1850, when the state of his health made it dangerous for him to go abroad in the evening. Such was the respect in which he was held by the Society, that although it was evident that he would not again preside at any of its meetings, it was unanimously agreed to make his case an exception to the rule lately adopted, limiting the period during which any President or Vice-President can be chosen to these offices.

Dr. Thomson died on the 2d of July last, and it falls to me, as the senior Vice-President, to lay before you some account of the man, whom, for so long a period, the Society has looked upon as its great honour and ornament. With this view I have not failed to renew my acquaintance with his works—originally with the intention of drawing up a

detailed report of his various labours—and it was not until I had nearly completed an enumeration of his publications, comprising as they do the results of fifty years of a most active literary and scientific life, that I found it necessary to circumscribe the undertaking.

To draw up a *Catalogue raisonnée* of such materials would not certainly have been difficult. Nor would it have been difficult to compose an *Eloge* of their author, as is frequently done in such circumstances, by pointing out passages in each writing, to illustrate his industry and his talent. That course would be the most grateful, as it is at first sight the most becoming in one who, like myself, has for many years enjoyed frequent and friendly intercourse with him; and it might also be the safest for the moment with a society which has so recently been deprived of an honoured head. But this would be an unphilosophical use to make of such accumulated results of industry, and in adopting it we should stray very widely from the example our President has left us in the graphic sketches which he drew of his predecessors and their works. There we find no symptom of that distemper with which biographers are so often afflicted, termed by Macaulay "the *Lues Boswelliana* or disease of admiration." Dr. Thomson was himself in little danger of yielding to the temptation, in such cases, to exaggerate, and would have despised the weakness which could lead into such a snare.

Before speaking more particularly of his works, I think it right on this occasion to give the Society a sketch of the personal history of Dr. Thomson; and along with what I myself know of his private life, I shall make use of the chronological and other statements, and even of the sentences when they suit me, which appeared in the Literary Gazette, and were thence copied into the Glasgow newspapers soon after his death. They are evidently from the pen of Dr. R. D. Thomson.

The subject of this memoir was the seventh child and the youngest son of John Thomson and Elizabeth Ewan, and was born at Crieff on the 12th of April, 1773. His education commenced at the parish school of Crieff, and in 1785 he was sent, for two years, to the borough school of Stirling, presided over at that time by Dr. Doig. Here he acquired a thorough classical education, and wrote a Latin Horatian poem, which attracted for him the attention of Professor M'Cormack of St. Andrews, as well as of his uncle the Rev. John Ewan, minister of Whittingham in East Lothian. By their advice he went to St. Andrews in 1787, and stood an examination in that University for a bursary which was open to public competition. He carried the scholarship, and was thereby entitled to board and lodging in the University for three years.

In 1791 he became tutor in the family of Mr. Kerr of Blackshiels. At the end of 1795, being desirous of studying medicine, he went to reside in Edinburgh with his elder brother, now the Rev. Dr. James Thomson of the parish of Eccles, who had succeeded the late Bishop Walker as colleague to Dr. Gleig in the editorship of the Encyclopædia Britannica. In the session 1795–96 he attended the chemistry class in

the University of Edinburgh, at that time taught by Dr. Black—a man of whom he could never speak without admiration. Although all Dr. Black's discoveries were made before 1766, when he left the chair of chemistry in Glasgow, the lectures he continued to give in Edinburgh were scarcely less remarkable, and were no doubt of immense advantage to Thomson. As published after his death by his friend Professor Robison, they contain an inexhaustible fund of information, and are wonderfully free of the errors of the time. During the session of his attendance at Dr. Black's lectures, Dr. Thomson wrote the article on the "Sea," for the Encyclopædia Britannica. In November 1796 he succeeded his brother in the editorship of the Supplement to the third edition of that work, and remained in this position till 1800. It was during this period that he drew up the first outline of his System of Chemistry, which appeared in the Supplement under the articles, "chemistry, mineralogy, vegetable substances, animal substances, dyeing substances." These all appeared before the year 1800, when the preface was published which contains the following remarks by Dr. Gleig. "Of the writer," he says, "of these beautiful articles, a man of like principles with Dr. Robison, it is needless to say anything, since the public seems to be fully satisfied that they prove their author eminently qualified to teach the science of chemistry." From this passage it may be inferred that it was during the winter of 1800–01, that Dr. Thomson first gave a course of lectures on chemistry. He was thus before the public as a lecturer for the long period of fifty-two years, and for some time before his death he had considered himself the oldest teacher in Europe. He graduated in 1799. "The self-taught chemist," says Dr. R. D. Thomson, "began to devise many of his views in a narrow close in the High-Street of Edinburgh; the author being in the receipt of a salary of £50 a-year, from which he sent £15 to his aged parents."

Dr. Thomson continued to lecture in Edinburgh till 1811, and during that time he opened a laboratory for pupils, probably the first established in Great Britain. Among those who worked there, was Dr. Henry of Manchester, who had visited Edinburgh for the purpose of graduation, and who there made his first experiments on the constituents of coal gas. During this period, Dr. Thomson likewise made investigations for government on the malt and distillation questions, and afterwards wrote the article "Distillation" in the Edinburgh Encyclopædia. He also invented his Saccharometer, still used, according to Dr. R. D. Thomson, by the Scottish excise, under the title of Allan's Saccharometer.

In 1812 Dr. Thomson published his history of the Royal Society, which might rather be called a Digest of the "Philosophical Transactions." The papers are arranged in it under distinct heads, according to the sciences to which they respectively belong. Every science is introduced by a history commencing with its origin, and traced down to the period of the establishment of the Royal Society. Sketches are also given of the lives of the most eminent of the contributors.

In August, 1812, having finished his history of the Royal Society, and being accidentally detained in Edinburgh without any specific employment, he took advantage of the peace just concluded with Sweden, and sailed for that country in company with his friend Mr. William Ritchie, of the High School of Edinburgh. He collected there much information on the natural and political history of the country, as well as on the state of its science, and published his observations in the following year.

On his return from Sweden he went to London, and projected the "Annals of Philosophy." He conducted that work during the six years of his residence in London, and for two years more after his removal to Glasgow. He then found himself obliged to resign the editorship in consequence of his distance from the place of publication, which quadrupled, as he said, the labour of the editor, and diminished almost in the same proportion, its successful exertion. The work was taken up by Mr. Richard Phillips in 1821, and in 1827 it was purchased by Mr. Richard Taylor, and merged in the Philosophical Magazine.

In 1817, on the death of Dr. Cleghorn, and at the recommendation of Sir Joseph Banks, Dr. Thomson was appointed lecturer on chemistry in the University of Glasgow, and in the following year, at the instance of the late Duke of Montrose, chancellor of the university, the appointment was made a regius professorship.

During the long period of twenty-three years, until the year 1841, Dr. Thomson discharged all the duties of his chair without assistance. Being then in his sixty-ninth year, and feeling his strength decline, he associated with him his nephew, Dr. R. D. Thomson, who was then resident in London, continuing himself to deliver the inorganic course till 1846. The dangerous illness of his second son, hurried him for the winter of that year to Nice, when his nephew was appointed by the University to discharge the entire duties of the chair, which he continued to do until his uncle's death.

In mentioning the termination of Dr. Robert Thomson's connection with the University of Glasgow, I mention also, I am sorry to say, that of his connection with this city, and with this Society, or at least of that intimate connection which he has had with the Society for so many years. Those who have taken the largest share in administering the affairs of the Society, know best its loss.

One cannot but remark the constant recurrence of evil which prevails in the Scottish Universities, from the want of a provision for the retirement of its superannuated Professors. Either is there an unseemly and injurious bargaining between the incumbent and his successor, with the connivance, of course, of the patron, preventing an open competition for the place about to become vacant, or the aged professor retains nominal possession of his chair after he is disqualified to discharge its duties. In the latter case a substitute is appointed, who may, or may not, act his part well. If, as is too likely to be the case, other than public motives guide the appointment, the University may

suffer from the class being inefficiently taught; and if, on the other hand, the substitute be found really worthy to become ultimately the professor, a meritorious teacher, perhaps in the middle of his days, is left, on the death of the incumbent, at the mercy of whoever may chance at the time to have the dispensation of the patronage. The system is, therefore, a mischievously defective one, and I make the remark at this time with the less reluctance, that neither has the substitute on the present occasion been unworthy of the succession, nor, on the other hand, can the new appointment, whatever may have been the motive of the minister, be considered otherwise than an excellent one. I will only add the hope that the subject may receive attention in quarters where there is power to apply the remedy.

Dr. Thomson continued to attend the examinations for degrees for some years after retiring from the duties of the chair, but in consequence of the increasing defect in his hearing, he ultimately gave up these examinations, and confined his public labours to attendance at the fortnightly meetings of the Philosophical Society. During the early part of the present year his frame became visibly weaker, and latterly, having removed to the country, where it was hoped the freshness of the summer air might brace his languishing powers, he breathed his last in the bosom of his family at his temporary residence at Kilmun.

Dr. Thomson married, in 1816, Miss Agnes Colquhoun, daughter of Mr. Colquhoun, distiller, near Stirling. He enjoyed uninterrupted happiness with her; and her loss in 1834, I well know, he deeply lamented. He has left one son, Dr. Thomas Thomson of the Bengal army, the author of Travels in Tibet, which have just appeared—the result of several years' researches into the botany and physical structure of the Himalaya mountains. He has left also one daughter, married to her cousin Dr. R. D. Thomson.

Of Dr. Thomson's personal character I can scarcely speak too highly. All who knew him must have remarked his manly independence—the unbending rectitude of the course which he invariably pursued—the sincerity displayed in all his intercourse—the readiness with which he gave his assistance when it was wanted. I agree most thoroughly, from personal observation, in all that has been said of the kindness of his disposition and the steadiness of his friendships; and I believe there is not one of his pupils who does not remember him with affection and esteem. More than twenty years since I asked him to name one of his pupils for a situation of some promise in Lancashire. He recommended a young man, who subsequently accepted the appointment, and who is now an extensive manufacturer. On being asked by a friend why he had not named a nephew of his own, who was also well qualified, Dr. Thomson answered that the other had a mother and a sister to support. That former pupil travelled from Manchester to follow the remains of his master to the grave.

Dr. Thomson's deportment to strangers, of which perhaps too much has

been said, was, I am persuaded, misunderstood in most of those cases where he left an unfavourable impression; for although little attentive to conventional usages, and decidedly sparing of complimentary language, he was well known to be remarkably sensitive to the feelings of others.

In ecclesiastical matters Dr. Thomson adhered to the Church of Scotland, and was, if I am not mistaken, a licentiate of that body. He took no part, however, in church politics, and although in articles of faith, as indeed on every other subject, he had fixed and well-considered opinions, he has never given them to the world; nor, so far as I know, has he made them known to friends out of his own family I was favoured some years ago, without Dr. Thomson's knowledge, with permission to copy out a Catechism of his composition, written in his own hand, for the use of his infant family. It proves the religious feeling, which none who knew Dr. Thomson doubted him the possession of, and it touches upon so many points in the Christian faith, on which the opinions of its author have scarcely been made known, as to call from a friend of mine in another church an expression of satisfaction at finding in the Catechism so much of which he could approve, and so little to condemn. The work is not certainly a "body of divinity," but, so far as it goes, it must be admitted to be excellent.

As a collector and compiler, I believe it will be generally granted that Dr. Thomson, particularly in the earlier part of his career, was unequalled. His System of Chemistry was looked upon for many years as the most complete and well arranged collection of facts, and it abundantly proved the prodigious industry and perseverance, as well as the extensive knowledge of its author. As a systematist he may have adhered too long to the arrangement which he founded on the combustibility of bodies, excellent as it was, and consistent with what was known at the time of its introduction, but in our earlier days Thomson's system was deservedly esteemed the standard work on chemistry, and was always referred to for the state of the science on any particular subject.

Of Dr. Thomson as a journalist, there can be no doubt that he did essential service to the progress of science, in commencing the "Annals of Philosophy." Nicholson's Journal, as well as the Philosophical Magazine, had then lost much of its interest for the student of chemistry. Dr. Thomson introduced many new features into his journal. Besides original papers contributed by his friends, he gave abstracts of the most important researches of the Continental as well as the British chemists, and accompanied them with remarks which, if themselves sometimes open to criticism, gave all the life and interest to the subject, which open discussion gives to the papers read at the meetings of a scientific society. He was the first, as his nephew has remarked, to introduce annual Reports of the progress of improvement in the natural sciences. These were commenced in 1814, and continued till 1820. In the collecting and arranging of materials for such a contribution, we can understand how an actively conducted journal keeps its editor au courant of the subjects of

which he treats, and in imagining such a work, we see how ready Dr. Thomson was to turn all his labours to account. In the "Annals" also were *Comptes rendus* of the proceedings of the Royal Society. Every chemical paper read there was reported with an exactness which will astonish any one who looks back at the array of figures and of facts, and who also knows that they were transcribed from memory alone. I have heard Dr. Thomson explain that the rules of the Society prevented the taking of notes with a view to publication before the appearance of the "Transactions," and that he consequently applied himself to the committing to memory even of numerical statements for insertion in the following number of the Annals of Philosophy. As soon as possible, after leaving the meeting, he transferred to paper what he had carried away, after which he could not have remembered more of it than the general facts.

I shall now relate the part Dr. Thomson took in the promulgation of the *Atomic theory*, and I shall do so at some length, as I think he has not received due credit for the share he had in the progress of that great work. I reckon this the most important proceeding of his life, unless we place before it his System of Chemistry, the influence of whose earlier editions it is difficult to estimate. On the 26th of August, 1804, Dr. Thomson went to Manchester, and saw for a day or two much of Mr. Dalton, who explained to him his views on the composition of bodies. He saw at a glance, as he tells us, the immense importance of such a theory, and was delighted with the new light which immediately struck his mind. He wrote down at the time the opinions which were offered, and three years later, when about to publish the third edition of his System of Chemistry, he obtained Dalton's permission to insert the sketch he had taken, before Dalton himself had given it to the world. The theory was at that time very slenderly supported by facts, for chemists possessed few experiments which could be considered as even approaching to accuracy. Up to the time when Thomson published the sketch, he seems to have been Dalton's only convert. Perhaps no other chemist had taken the trouble to listen to it, if we except Dr. Henry of Manchester, who was Dalton's frequent visitor, but there is no probability that even he at so early a period accepted the theory, for he speaks of it, so late as 1810, in rather doubtful terms, in the sixth edition of his "Elements."

Thomson's paper on the oxalates, read to the Royal Society in 1807, contained the first direct example of the application of the Daltonian theory to supersalts. He there shows that oxalic acid unites with strontian as well as with potash in two different proportions, and that the quantity of acid combined with each of these bases in their superoxalates, is just double of that which saturates the same quantity of base in their neutral compounds. During the same year Dr. Wollaston read his famous paper on the oxalate, binoxalate, and quadroxalate of potash, and he commences it with a relation of what Thomson had already done. He *states that* he had remarked the same law to prevail in various other

instances of superacid and subacid salts, and that he had intended to pursue the subject so as to learn the cause of so regular a relation; but that such a pursuit was rendered superfluous by the appearance of Dalton's theory, as explained and illustrated by Thomson. He shows also that the bicarbonate of soda loses one-half its carbonic acid by exposure to a red heat—that the potash in supersulphate of potash is united to twice as much acid as the same quantity of potash in the neutral sulphate, and that potash unites with three different quantities of oxalic acid, which bear to each other the relation of 1, 2, and 4. Dr. Thomson always said, that in the absence of Dalton, Wollaston would have been, very soon, the discoverer of the atomic theory.

These facts gradually drew the attention of chemists to Mr. Dalton's views. Sir Humphry Davy, however, and others of our most eminent chemists, were hostile to them. In the autumn of 1807, Dr. Thomson had a long conversation with Mr. Davy at the Royal Institution, during which he attempted in vain to convince him that there was any truth in the new hypothesis. A few days after, he dined with him at the Royal Society Club at the Crown and Anchor in the Strand. Dr. Wollaston was also present. After dinner every member left the tavern, except Dr. Wollaston, Mr. Davy, and himself, who all remained behind, and sat an hour and a-half conversing upon the atomic theory. Wollaston and Thomson tried to convince Davy of the inaccuracy of his opinions; but he went away more prejudiced than ever. Soon after, Davy met Mr. Davies Gilbert, the President of the Royal Society, and exhibited to him the atomic theory in so ridiculous a light, as to make Mr. Gilbert call afterwards on Dr. Wollaston, to learn, probably, what could have induced a man of his sagacity and caution to adopt such opinions. Dr. Wollaston begged of Mr. Gilbert to sit down and listen to a few facts which he would state to him. He then went over the principal facts, at the time known, respecting the salts in which the proportion of one of the constituents increases in a regular ratio; and the relations also which Dalton had found carbon to bear to hydrogen in olefiant gas and carburetted hydrogen. Mr. Gilbert went away a convert to the truth of the atomic theory, and had soon the merit of convincing Sir Humphry Davy, who ever after was a strenuous supporter of it.

This incident is related in Dr. Thomson's "History of Chemistry," published in 1831—one of the most delightful books that can be read by a zealous chemist. It is full of biography and anecdote connected with chemistry and chemists. The lives of most of the moderns are taken, with little alteration, from the Annals of Philosophy, where he had first published them, and the series is there completed, not of course with the same originality, but with prodigious industry and great discrimination, accompanied also with interesting additions and criticisms of his own. The book was published in the modest form of a contribution in two volumes to the "National Library." It has never appeared in any other form, and if Messrs. Colburn and Bentley could induce Dr

R. Thomson to superintend a new edition, completing it with a memorial, such as he might write, of its illustrious author, they would produce one of the most interesting works that could be given to scientific men.

Instead of Dalton's term "atom," which Thomson adopted, Davy always used the word "proportion," and Wollaston "equivalent," which was much better; but whatever term we employ, now that the thing itself is understood, there can be no doubt that the use of the word "atom," (which conveys at once the idea of an ultimate indivisible particle,) greatly contributed to the reception of the doctrine of definite proportions. In 1808 Mr. Dalton published a volume of his own, in which not more than five pages, widely printed, and one plate with explanations, were devoted to the announcement and illustration of the atomic theory. This treatise, if such it can be called, is little more copious than that which had been given the year before from Dr. Thomson's notes.

In 1809, Gay Lussac made known his theory of volumes. In 1810, as I understand, Berzelius first published his "Essay on the Cause of Chemical Proportions," and it was translated by Dr. Thomson for the Annals of 1813–14. It contains a determination of the constituents of many important bodies. It shows that when a sulphate is formed from a sulphite or a proto-sulphuret, the sulphate is always neutral, and that in compounds of acids and bases, containing oxygen, the acid contains 2, 3, 4, 5, &c. times as much oxygen as the base. In 1813, Dr. Thomson commenced in the Annals an elaborate treatise on the Daltonian theory, and appended to it an extensive list of atomic weights. This he was enabled to do by comparing together the results of experiments which had recently been undertaken by Davy, Gay Lussac, Biot and Arago, Berzelius, Wollaston, and others, on the specific gravities of gases and the composition of solid bodies. The numbers for the elementary bodies are exceedingly near the truth. They attracted the notice of Dr. Prout, and in November, 1815, that chemist announced anonymously his celebrated doctrine, that the atomic weights of all bodies, solid as well as gaseous, are multiples of the atomic weight of hydrogen.

The view taken by Dr. Prout was not an arbitrary one, guessed at from the accidental approach to it of a few of the elements. The discovery of Gay Lussac, that gases unite together in exactly equal or multiple volumes, and the circumstance that the specific gravities of several of the gases had been already ascertained (before 1815) with great precision, led directly, as far as these gases were concerned, to that conclusion. There were means also for taking the specific gravities of solid bodies in the gaseous state, as carbon in the state of carbonic acid, sulphur in sulphurous acid, &c., and the application of the principle to other solid substances, such as potassium, calcium, or iron, although none of their compounds can be produced in the state of gas, was apparently inevitable. Round numbers were thus obtained for several of the elements. Carbon, for example, had an atomic weight six times that of hydrogen. Oxygen, as it united with twice its volume of hydrogen to form

water, and having exactly sixteen times its density in the state of gas, had an atomic weight of 8. In the same way he gave 14 for azote and for phosphorus, 16 for sulphur, 20 for calcium, 24 for sodium, 28 for iron, 32 for zinc, 36 for chlorine, 40 for potassium; and thus he obtained not only round numbers as compared with hydrogen, but numbers which when divided by 2, and most of them by 4, were still multiples by even numbers, and without residue, of the weight of hydrogen.

Thomson was again the first to perceive the truth and the importance of the discovery made by Dr. Prout. He immediately adopted it, and in November, 1818, he published a new table of atomic weights, embodying its principles, and taking advantage of all the improvements that had been made in analysis during the previous five years.

In the meantime Berzelius, by a long course of the most persevering exertion, had obtained experimental results of great exactness and great value, from an immense number of bodies. Many of them were published in 1813, and others followed. When Thomson published his illustrations of the doctrine of Prout, Berzelius refused to accept it, or to be guided otherwise than by the results of his experiments. The numbers of Berzelius were adopted almost universally on the continent, and partially even in this country. In most instances they differed but slightly from Thomson's numbers. In others, however, the difference was considerable, as in the very important case of carbon, where it was nearly 2 per cent. It was not till 1840, that any chemist of note joined Thomson in the defence of Prout's doctrine. During these twenty-five years, he maintained his principles and the correctness of his numbers almost single-handed, for, as in the case of Dalton, Prout had done a great proportion of his work when he announced his theory. The experiments of Biot and Arago, which guided Wollaston, Thomson, and Prout, gave 75·4 as the atomic weight of carbon, oxygen being 100; and Prout, on the theoretical considerations I have mentioned, reduced it to 75, which is equal to 6 on the hydrogen scale. But Berzelius, conjointly with Dulong, had obtained a different experimental result, and, in accordance with it, his number for carbon was 76·438.

At length, however, in December, 1840, a most important paper was read to the Academy of Sciences, by MM. Dumas and Stas, on the quantity of carbonic acid produced by the burning of carbon. Graphite, after undergoing a process of purification, was taken for one set of experiments, and diamond for another. These substances were burned in a modification of the tube for organic analysis, and the carbonic acid was collected in a Liebig's alkali-apparatus. The details are exceedingly interesting, but I must not repeat them here. Several months were spent on this single experiment, and nearly half-an-ounce of diamond burned. The results from graphite gave

For the atom of carbon, ...74·982
And those from diamond gave, ...75·005

The mean of the whole being.....74·993

The experimenters purposely kept themselves ignorant of the weight of the diamond they had employed, until the results were calculated. On one occasion they concluded that 708 milligrammes had been burned, when in reality 717 had been weighed to them. They examined the tube which had contained the diamond, and found there the deficiency—equal to 9 milligrammes—in fragments of Brazilian topaz. These experiments were repeated the following year by MM. Erdmann and Marchand. They obtained a result nearly similar, viz. 75,087 for the atom of carbon. The French as well as the German chemists concluded that 75 was the real number for carbon.

In 1843, M. Dumas made known his adherence to the atomic weights of three more of the elements on Prout's list, viz. hydrogen 0·125, azote 1·75, and calcium 2·50. Erdmann and Marchand again corroborated his results for hydrogen and calcium, and determined anew the number for mercury to be 12·5, and for sulphur 2·0, oxygen being 1·00. Svanberg and Norlin found the number for iron to be exceedingly near to 3·5, a result which was confirmed by Berzelius himself. Marignac found, for azote, the number 1·7525, and Anderson 1·744 from his experiments on the nitrate of lead; close approximations on either side of 1·75. It must be admitted, however, that Thomson's numbers for chlorine, potassium, sodium, and a few others of the elements still want confirmation. There seems little doubt, then, (and these experimenters incline to the opinion,) that the doctrine of multiples of hydrogen is a law of nature, and that the more exact we become in our analyses, the more shall we approach to it in our results. These are the precise numbers which Prout originally determined, and Thomson for twenty-five years maintained, against much opposition; and seldom do ingenuity, sagacity, and perseverance meet with so successful and triumphant a conclusion. I remember having had the chance to announce to Dr. Thomson one of these substances, I forget which, as having dropped into his list. I need not tell his friends that no expression of triumph or of gratification escaped him. He took the result as a matter of course, and was confident, that for other numbers also, it was only a question of time.

To give an account of Dr. Thomson's own contributions to Nicholson's Journal, to the Philosophical Transactions, to the Annals of Philosophy, to the Records of Science, and to the Proceedings of our own Society, would be to take in review his merits as an investigator more fully than is consistent with my present plan. I will only say that they have their excellences and they have their wants. Other men have observed more of the hidden characters of the substances on which they worked; and, considering the multitude of experiments Dr. Thomson has conducted, he has certainly made known to us comparatively few important new bodies.

In 1804, in a paper on the oxides of lead, Dr. Thomson first introduced the use of the Greek ordinal numbers to denote the degree of oxidation of a metal. Thus, the protoxide is that in which the metal is united to a minimum of oxygen; the deutoxide has the second degree of oxidation;

the tritoxide the third, and so on—the term peroxide being given to that in which the metal is united to a maximum of oxygen. When it was discovered that in a supersalt the proportion of acid to the base was twice as great as in the neutral salt, Dr. Wollaston, to denote that relation, prefixed the Latin word *bis* to the acid of the supersalt. In the same way he gave the name quadroxalate to a salt of potash in which the proportion of acid to the base was four times as great as in the neutral salt. When the base is doubled, Dr. Thomson denotes this by prefixing to the name of the acid the Greek syllable *dis*; and for salts having three and four times the proportion of base, *tris*, *tetrakis*, &c.

As an analyst, Dr. Thomson, by his earlier experiments, which are exceedingly numerous, contributed greatly to the advancement of the science, when it needed such information. His analyses professed to be nothing more than approximations to the truth, and they served their purpose well. But after the atomic theory was established, and the question became, to determine the weights of the equivalents of bodies, a degree of precision was required, which till then had been practised in but few cases, and which indeed the means at their disposal seldom enabled chemists to attain. After the part which, as we have seen, Dr. Thomson took in promulgating the principles of Dalton and of Prout, he undertook a great amount of labour for the purpose of establishing, by experiment, the weights of the elements. His experiments on the specific gravities of the gases confirmed most exactly the numbers of Prout. They were performed, with the assistance of Mr. Alexander Harvey, with great care—minute particulars are given of the methods employed; and the series attracted none of the severe animadversions that have been bestowed upon the analyses of the salts.

The results of five years' labour upon the salts were published in 1825 in the two volumes forming the "Attempt to establish the First Principles of Chemistry by Experiment." The principal method employed was that by double decomposition, used with great advantage by Wenzel. A quantity of muriate of barytes, for example, was weighed and dissolved in distilled water. A quantity of sulphate of potash, nearly an equivalent of the first, was also weighed and dissolved. The two solutions were mixed together and filtered, and the liquid was tested for barytes and for sulphuric acid. If barytes was indicated, the mixture was made anew, with an additional portion of sulphate, and *vice versa*—the process being repeated until a point was found at which no precipitate occurred on the addition of either salt. The atomic weight of one of the salts being known, the experiment decided that of the other. No attempt was made to collect the two new salts formed in the process. The proportions of the constituents of a great number of salts were thus determined; and here the confirmation given to the original numbers of Dr. Prout, was not only complete, but if taken literally, was altogether marvellous.

It is to be regretted that the examination of this work should not have been conducted in a better spirit. There were, no doubt, causes of irrita-

tion on the part of Berzelius. He felt his authority to be as great as that of Dr. Thomson, and with a temper more easily ruffled, he could not be expected easily to bear the commendations and the censures that had for years been awarded him by the Scottish chemist, according to the view he took of his results. In the preface, also, to the "First Principles," there may have appeared to Berzelius an excess of confidence on the part of its author in the importance of his work, and something like an undervaluing and setting aside of previous determinations. But nothing can justify the language employed by Berzelius in the "Report" which he published annually, and in which he also was in the habit of distributing judgment with more freedom than could always be received with equanimity. I shall not repeat the expressions; but in touching the moral character of Dr. Thomson, as if he had purposely invented results, he showed how little he knew the man. Dr. Thomson was incapable of deceiving others when not himself deceived, and that is the question alone worthy of our attention.

It was evidently the opinion of other chemists as well as of Berzelius, that Dr. Thomson would not have reached the same perfection without previously knowing the exact results he ought to obtain, supposing the substances on which he operated to have been absolutely pure. How this may be true, without taking from the genuineness of the experiments to which the statement refers, is now understood by every practical chemist. Dr. Thomson, as is well known, followed the example of Wollaston in taking the atomic weight of oxygen for unity. Hydrogen consequently became 0·125, and the rule he had laid down for his guidance from Prout, enabled him to decide that the atomic weight of every other element, and consequently of all bodies, must be a number divisible without remainder, by 0·125, the atomic weight of hydrogen. It is thus that an experimenter obtains a standard by which to test the accuracy of his results, and it is only after repeated preliminary experiments, and a continued reference to the table of equivalents, that he can be certain of the removal of all his errors.

In every case of more than usual precision, however, and particularly in all debated cases, the chemist is expected to state every step in his progress. He must tell the difficulties he has met with, and how they have been obviated. Above all, he must satisfy other chemists that he knew the purity, or the degree of purity of his materials, and especially their hygrometric state at the moment when they were weighed.

Now it is a very general opinion that Dr. Thomson did not give the details necessary to inspire confidence in the accuracy of his results. He has not even described the methods by which he prepared some substances, which, as we know, can only be produced in a state of purity by tedious and difficult processes. The results, as they are stated, are at the same time unusually and perplexingly exact. The consequence is, that the work has exercised little of the influence which appears to have been expected from it by its author.

It is but lately that I noticed in a paper on sulphate of zinc, published by Dr. Thomson soon after the appearance of his work, that he freely admits the scantiness of detail, and that he accounts for it in a manner which leads, I think, to an answer to the whole question. "I abstained," he says, "from describing the processes which I followed, because I thought them rather too tedious for a work of the nature that I had projected; and because it was in my power, in a book intended chiefly for my own students, to supply verbally whatever was wanting in the practical part." I have also the impression, from that paper, and from a review of the work itself, notwithstanding some appearances to the contrary, that the results, which appear so perfect in the "First Principles" are not to be understood as the actual results of any one experiment, or even as the mean of several experiments, but rather as results which might fairly be deduced from them; and which, being in round, as well as more perfect numbers, were more suitable for a school book. Had Dr. Thomson been more explicit in the work itself, he would have been saved much annoyance, and chemists would have known that the experiments he related were undertaken, and described, more as instructions to his pupils than as contributions to the science.

It is not without considerable difficulty that I have been enabled thus to reconcile the remarks, not always unjust, which have been made upon this work, with the faithfulness which distinguished all the statements of its author; and I have entered into the subject on the present occasion the more fully, and with the less hesitation, from being convinced that the appearance of failure in a work like this, which demanded greater delicacy of manipulation than I believe him to have possessed, and a keener eye for possible slight inaccuracies, has acted prejudicially and most unjustly upon his reputation in other departments where these qualities are not so essential, and in which he stands pre-eminent.

I would not be understood as denying the existence of positive errors in the experiments described in the "First Principles." The mode of analysis, as it is there related, where a salt of barytes is decomposed by one containing sulphuric acid, is itself liable to objection; and certainly nothing like accuracy can be expected from a similar experiment with a salt of lead. The chapter on the salts of alumina might also be instanced as defective.

It is to Dalton that we are indebted for the first proper application of Symbols to chemical science. With him a circle represented an atom of oxygen—a circle enclosing a dot was an atom of hydrogen—a circle with a line an atom of azote, and so on. Thomson indeed had long before used initial letters to denote the composition of minerals; as A for alumina, L for lime, S for silica, and A M G for a mineral containing alumina, magnesia, and glucina. These occur in his article "Mineralogy" which appeared in 1798 in the Encyclopædia Britannica, but they indicated no particular quantity of the substances they represented, and are scarcely worthy of mention in this sketch. In the very first paper, how-

ever, that was published after Dalton, in illustration of his theory,* Dr. Thomson not only substituted alphabetical symbols for the circles of Dalton, but he employed them in the construction of formulæ, for the purpose of building up and picturing to himself the composition of oxalic acid, as ascertained by the substances obtained during the application of heat to oxalate of lime.

"Let an atom of oxigen," he says, "be *w*, an atom of carbon *c*, and an atom of hidrogen *h*. An atom of oxalic acid may be represented by $4\,w + 3\,c + 2\,h$." "Three particles of oxalic acid resolve themselves into these substances in the following proportions:—

"4 particles of carbonic acid, ...	=	$8\,w + 4\,c$.
2 particles of carburetted hidrogen,	=	$2\,c + 4\,h$.
2 particles of carbonic oxide, ...	=	$2\,w + 2\,c$.
2 particles of water, ...	=	$2\,w + 2\,h$.
1 particle of charcoal, ...	=	$1\,c$.
Total, ...		$12\,w + 9\,c + 6\,h$.
3 particles of oxalic acid, ...	=	$12\,w + 9\,c + 6\,h$."

"Sugar," he says, "is a compound of 12 atoms, namely: five of oxigen, three of carbon, and four of hidrogen; the weight of an integrant particle of it is 47·5, and its symbol is $5\,w + 3\,c + 4\,h$." Berzelius greatly extended the use of these symbols, but he did not claim the merit of having introduced them in chemical investigations. Dr. R. D. Thomson, in the paper to which I have already referred, quotes a passage to this effect from a work of Berzelius published in Swedish in 1814, where he says that he strictly "followed the rules for this purpose given by Thomson in his system of chemistry" (och skall dervid folga en enledning som Thomson gifvit i sin kemiska handbok.) "The work," continues Dr. R. D. Thomson, "in which this passage occurs, entitled 'Forsok att genom användandet af den electrokemiska theorien &c., grundlägga for mineralogier' af J. Jacob Berzelius, Stockholm, 1814, page 18, was sent by Berzelius to Dr. Thomson in the same year, with a request, in a letter which is still extant, that he would endeavour to procure a translator for it. Dr. Thomson applied to Dr. Marcet and others without success, but at last prevailed on his learned friend, John Black, Esq., who so ably conducted the Morning Chronicle newspaper for many years, to undertake the task."

It is not claiming, then, more for Dr. Thomson than his due, when we say, notwithstanding the extensions and improvements of Berzelius, that he was the inventor of the use of symbols as they are now employed in chemical language.

At a future time, if the Society think fit, I shall lay before it some account of Dr. Thomson's biographical works. His Lives of the Chemists must always be read with interest.

* Philosophical Transactions for 1807, page 63.

November 17, 1852.—Mr. Crum, *Senior Vice-President, in the Chair.*

The following were proposed as members, viz. :—Thomas Anderson, Professor of Chemistry, Mr. James Young, (Edinburgh), Mr. Edward Meldrum, (Bathgate), and Mr. William Nielson.

The second vote of the Society was taken on the change in the law limiting the President's tenure of office to two years at a time, and finally agreed to.

The Society then proceeded to the fifty-first annual election of its office-bearers.

Mr. Robert Blackie called attention to the importance of the votes of the Society being more concentrated than heretofore, inasmuch as the law regulating the eligibility of members of Council by the number of votes recorded for them was now about to come into operation. In order to prevent the votes from being diffused over a large proportion of the entire list of members, he proposed that twelve or more names be written on the black board, after being moved and seconded, and that it be optional for members to add to the number, and otherwise to vote as they chose.

This proposal having been submitted to the Society, permission was given to write the names on this occasion.

A list of names having been moved and seconded, was then written on the black board by the mover. Several additional names were proposed by other members, and also written upon the board.

The voting then took place in the usual manner. Mr. William Ramsay and Mr. Donald Campbell were requested to act as scrutineers of the votes. The scrutineers having retired to examine the vote papers,

Mr. W. J. Macquorn Rankine read a paper "On Telegraphic Communication between Great Britain and Ireland, by Mr. W. J. Macquorn Rankine and Mr. John Thomson."

"The authors maintain that the best route for a Submarine Electric Telegraph between Great Britain and Ireland is by the Mull of Cantyre; the breadth of the channel between that headland and Tor Point on the coast of Ireland being less than 13 miles, while the breadth between Portpatrick and Donaghadee is 22 miles; while in the former strait, the exposure is so great as to render it almost impossible for vessels to endanger the telegraphic cable by anchoring across it.

"They contend that on grounds of national advantage, this line of telegraph ought to be made, even supposing those by Holyhead and Portpatrick to be in operation.

"Besides its general utility, this scheme would be fraught with great local benefit to the west of Scotland and north-east of Ireland."

Mr. Rankine exhibited several specimens of submarine telegraphic cables manufactured by Messrs. R. S. Newall and Company. The copper conducting wires, each imbedded in a thick cord of gutta percha, are twisted together, wound round with rope-yarn, tarred, and finally

encased in the centre of an iron cable composed of wires of from one-eighth to one-quarter of an inch in thickness, according to the strength required.

Mr. Cockey, on the part of Dr. Robert D. Thomson, gave in the following report on the state of the Library at the period when he ceased to be Librarian:—

The Library has this year been carefully examined by Mr. James M'Lagan, (who has discharged his duties of giving out and receiving books since his appointment with great efficiency,) and has been found free from defects. The total number of volumes is 2070, showing an increase during the last year of 150 vols.; that in November, 1851, being 1920.

TABLE OF NUMBER OF VOLUMES AND NUMBER OF READERS MONTHLY.

	No. of Readers.				No. of Vols.		
	1851.		1852.		1851.		1852.
January,	42	...	50	.....	127	...	163
February,	44	...	48	.....	116	...	134
March,	34	...	57	.....	87	...	161
April,	45	...	49	.....	157	...	170
May,	46	...	36	.....	138	...	113
June,	38	...	43	.....	120	...	134
July,	35	...	37	.....	94	...	96
August,	37	...	32	.....	112	...	81
September,	41	...	35	.....	150	...	120
October,	44	...	33	.....	140	...	90
November,	45	...	—	.....	162	...	—
December,	50	...	—	.....	147	...	—

From this table it might be supposed that in some months the number of vols. read and the readers had diminished. This circumstance appeared to depend in some measure on the regulations in returning books, within the time specified by the rules of the Society. The second series of the Annales de Sciences Naturelles, the Quarterly Journal of the Royal Institution, and the second series of the Annals of Philosophy, have been completed during this year. The journals now requiring to be completed, are principally the Annales de Sciences, first series, Poggendorff's Annalen, Annales de Chemie, first series, Annales de Mines, early series, Journal de Pharmacie. It is a pleasing duty on the part of the Curator to be able to hand over such a valuable and thriving library to his successor, remembering, as he does, eleven years ago, when the volumes were comprised in a small cupboard, under one of the windows of the Andersonian Library—the number of ordinary members of the Society then numbering about 80, while at present they amount to 273.

Mr. Liddell gave in the Treasurer's Account, which was ordered to be engrossed as follows:—

Abstract of Treasurer's Account.

1851. DR.

Nov. 1.—To Cash in Union and Savings Banks,	£128	15	3				
1852.							
Nov. 1. — Interest on do.	3	4	9				
				£132	0	0	
To Society's Transactions sold,				1	14	0	
— Entries of 17 New Members, at 21s.	17	17	0				
— 11 Annual Payments from Original Members, at 5s.	2	15	0				
— 256 Annual Payments, at 15s. each,	192	0	0				
				212	12	0	
— Rent from Sabbath School Teachers, for use of Hall,				0	10	0	
				£346	16	0	

1852. CR.

Nov. 1.—By New Books and Binding,	£15	0	0	£100	5	0
— Printing Transactions, Circulars, &c.				34	17	0
— Wright Work, &c. in Hall,				7	4	0
— Rent of Hall, One Year,						
— Coffee, &c., at Annual Meeting,	2	6	6			
— Fire Insurance,	2	16	0			
— Society's Officer and Clerk,	6	0	0			
— Postages and Delivering Letters,	11	10	6			
— Stationery and Die Stamp,	4	15	6			
— Gas for Hall,	1	2	0			
				43	10	6
— Librarian's Salary, One Year,	20	16	0			
— Librarian for Poundage Collecting Dues,	6	14	4			
				27	10	4
— Subscription to Ray Society,	1	1	0			
— Do. to Cavendish Society,	1	1	0			
— Do. to Palæontographical Society,	1	1	0			
				3	3	0
— Painting the Hall,				7	9	0
— Balance—						
Cash in Union Bank,	120	0	0			
Do. in Savings Bank,	3	10	0			
				123	0	10
				£346	16	0

THE PHILOSOPHICAL SOCIETY EXHIBITION FUND.

1851.

May 15.—To balance, as per deposit receipt, from the Corporation of the City of Glasgow,				£551	0	9

1852.

May 15.—To One Year's Interest on do. 22 0 9

£573 1 6

GLASGOW, 1*st November*, 1852.—We have examined the Treasurer's Account, and compared the same with the Vouchers, and find that there are in the Union Bank of Scotland One Hundred and Twenty Pounds, and in the Savings Bank Three Pounds and Tenpence—together, One Hundred and Twenty-three Pounds and Tenpence—at the Society's credit.

The Treasurer has also exhibited to us a Voucher which he holds for money lent to the Corporation of the City of Glasgow, from the proceeds of the Philosophical Society's Exhibition in 1846, with Interest thereon up to 15th May ultimo, being £573 1s. 6d.

THOMAS DAWSON.
RICH. S. CUNLIFF.

Report by Treasurer, 3d November, 1852.—The property possessed by the Society at this date consists of the above-named Balance of £123 0s. 10d. in Bank; the Books in Library, and Book Presses, as per Librarian's Catalogue. The Furniture, Picture, Bust, &c., remain same as in last Report.

The number of Members admitted, Session 1851-52, and who have paid dues, is 17. The ordinary list has been reduced 4 by death, 8 by resignation, 4 from arrears in payment of dues, 7 from being placed on non-resident list, by desire, having paid arrear of dues, and 1 elected an Honorary Member; making in all 24. The total number on roll at this date is 281; of these, 24 are in arrear of dues for one year.

The scrutineers having now returned gave in the following report of the result of the election:—

President.

MR. WALTER CRUM.

Vice-Presidents.

DR. GEORGE A. WALKER ARNOTT. | DR. ALLEN THOMSON.

TREASURER,.....MR. ANDREW LIDDELL.
LIBRARIAN,.....MR. WILLIAM COCKEY.

Joint Secretaries.

MR. ALEXANDER HASTIE, M.P. | MR. WILLIAM KEDDIE.

Council.

MR. ALEXANDER HARVEY.
DR. A. K. YOUNG.
MR. WILLIAM GOURLIE.
MR. JAMES BRYCE.
PROFESSOR W. THOMSON.
MR. JAMES R. NAPIER.
DR. JOHN STRANG.
MR. WILLIAM MURRAY.
MR. ROBERT BLACKIE.
MR. NEIL ROBSON.
MR. J. MACQUORN RANKINE.
DR. ARTHUR MITCHELL.

December 1, 1852.—*The* PRESIDENT *in the Chair.*

THE following gentlemen were elected members of the Society, viz:—Thomas Anderson, Mr. James Young, Mr. Edward Meldrum, Mr. William Nielson.

The following were proposed as members, viz.:—Mr. John L. Dunn, Mr. Robert Mackay, Mr. William Boyd, Mr. William Broom, Mr. Andrew Jackson, and Mr. Edward Meikleham.

Professor William Thomson gave an "Account of Experiments by Mr. Joule and Professor William Thomson, on the Changes of Temperature occasioned by the Rushing of Air through Small Apertures."

Professor William Thomson afterwards read a paper "On the Economy of Heating or Cooling Buildings by means of Currents of Air."

XXXIII.—*On the Economy of the Heating or Cooling of Buildings by means of Currents of Air.** By PROFESSOR W. THOMSON.

IF it be required to introduce a certain quantity of air at a stated temperature higher than that of the atmosphere into a building, it might at first sight appear that the utmost economy would be attained if all the heat produced by the combustion of the coals used were communicated to the air; and in fact the greatest economy that has yet been aimed at in heating air or any other substance, for any purpose whatever, has had this for its limit. If an engine be employed to pump in air for heating and ventilating a building (as is done in Queen's College, Belfast), all the waste heat of the engine, along with the heat of the fire not used in the engine, may be applied by suitable arrangements to warm the entering current of air; and even the heat actually converted into mechanical effect by the engine, will be reconverted into heat by the friction of the air in the passages, since the overcoming of resistance depending on this friction is the sole work done by the engine. It appears therefore that whether the engine be economical as a converter of heat into mechanical work, or not, there would be perfect economy of the heat of the fire if all the heat escaping in any way from the engine, as well as all the residue from the fire, were applied to heating the air pumped in, and if none of this heat were allowed to escape by conduction through the air passages. It is not my present object to determine how nearly in practice this degree of economy may be approximated to; but to point out how the limit which has hitherto appeared absolute, may be surpassed, and a current of warm air at such a temperature as is convenient for heating and ventilating a building may be obtained mechanically, either by water power without any consumption of coals, or, by means of a steam engine, driven by a fire burning actually less coals than are capable of generating by their combustion the required heat; and secondly, to

* Mathematical demonstrations of the results stated in this paper have since been published in the Camb. and Dub. Math. Journal, Nov. 1853.

show how, with similar mechanical means, currents of cold air, such as might undoubtedly be used with great advantage to health and comfort for cooling houses in tropical countries,* may be produced by motive power requiring (if derived from heat by means of steam engines), the consumption of less coals perhaps than are used constantly for warming houses in this country.

In the mathematical investigation communicated with this paper, it is shown in the first place, according to the general principles of the dynamical theory of heat, that any substance may be heated thirty degrees above the atmospheric temperature by means of a properly contrived machine, driven by an agent spending not more than about $\frac{1}{33}$ of energy of the heat thus communicated; and that a corresponding machine, or the same machine worked backwards, may be employed to produce cooling effects, requiring about the same expenditure of energy in working it to cool the same substance through a similar range of temperature. When a body is heated by such means, about $\frac{32}{33}$ of the heat is drawn from surrounding objects, and $\frac{1}{33}$ is created by the action of the agent; and when a body is cooled by the corresponding process, the whole heat abstracted from it, together with a quantity created by the agent, equal to about $\frac{1}{32}$ of this amount, is given out to the surrounding objects.

A very good steam engine converts about $\frac{1}{10}$ of the heat generated in its furnace into mechanical effect; and consequently, if employed to work a machine of the kind described, might raise a substance thirty degrees above the atmospheric temperature by the expenditure of only $\frac{1}{33}\frac{0}{}$, or $\frac{3}{7}$, that is, less than one-third of the coal that would be required to produce the same elevation of temperature with perfect economy in a direct process. If a water-wheel were employed, it would produce by means of the proposed machine the stated elevation of temperature, with the expenditure of $\frac{1}{33}$ of the work, which it would have to spend to produce the same heating effect by friction.

The machine by which such effects are to be produced must have the properties of a "perfect thermo-dynamic engine," and in practice would

* The mode of action and apparatus proposed for this purpose differs from that proposed originally by Professor Piazzi Smyth for the same purpose, only in the use of an egress cylinder, by which the air is made to do work by its extra pressure and by expansion in passing from the reservoir to the locality where it is wanted, which not only saves a great proportion of the motive power that would be required were the air allowed simply to escape through a passage, regulated by a stop-cock or otherwise, but is absolutely essential to the success of the project, as it has been demonstrated by Mr. Joule and the author of this communication, that the cold of expansion would be so nearly compensated by the heat generated by friction, when the air is allowed to rush out without doing work, as to give not two-tenths of a degree of cooling effect in apparatus planned for 30 degrees. The use of an egress cylinder has (as the meeting was informed by Mr. Macquorn Rankine), recently been introduced into plans adopted by a committee of the British Association appointed to consider the practicability of Professor Piazzi Smyth's suggestion, with a view to recommending it to government for public buildings in India.

be either like a steam engine, founded on the evaporation and re-condensation of a liquid (perhaps some liquid of which the boiling point is lower than that of water), or an air engine of some kind. If the substance to be heated or cooled be air, it will be convenient to choose this itself as the medium operated on in the machine. For carrying out the proposed object, including the discharge of the air into the locality where it is wanted, the following general plan was given as likely to be found practicable. Two cylinders, each provided with a piston, ports, valves, and expansion gearing, like a high-pressure double-acting steam engine, are used, one of them to pass air from the atmosphere into a large receiver, and the other to remove air from this receiver and discharge into the locality where it is wanted. The first, or ingress cylinder and the receiver, should be kept with their contents as nearly as possible at the atmospheric temperature, and for this purpose ought to be of good conducting material, as thin as is consistent with the requisite strength, and formed so as to expose as much external surface as possible to the atmosphere, or still better, to a stream of water. The egress cylinder ought to be protected as much as possible from thermal communication with the atmosphere or surrounding objects. According as the air is to be heated, or cooled, the pistons and valve gearing must be worked so as to keep the pressure in the receiver below, or above, that of the atmosphere. If the cylinders be of equal dimensions, the arrangement when the air is to be heated, would be as follows:—The two pistons working at the same rate, air is to be admitted freely from the atmosphere into the ingress cylinder, until a certain fraction of the stroke, depending on the heating effect required, is performed, then the entrance port is to be shut, so that during the remainder of the stroke the air may expand down to the pressure of the receiver, into which, by the opening of another valve, it is to be admitted in the reverse stroke; while the egress cylinder * is to draw air freely from the receiver through the whole of each stroke on one side or the other of its piston, and in the reverse strokes first to compress this air to the atmospheric pressure (and so heat it as required), and then discharge it into a pipe leading to the locality where it is to be used. If it be required to heat the air from 50° to 80° Fahr., the ratio of expansion to the whole stroke in the egress cylinder would be $\frac{18}{100}$, the pressure of the air in the receiver would be $\frac{82}{100}$ of that of the atmosphere (about 2·7 lbs. on the square inch below the atmospheric pressure), and the

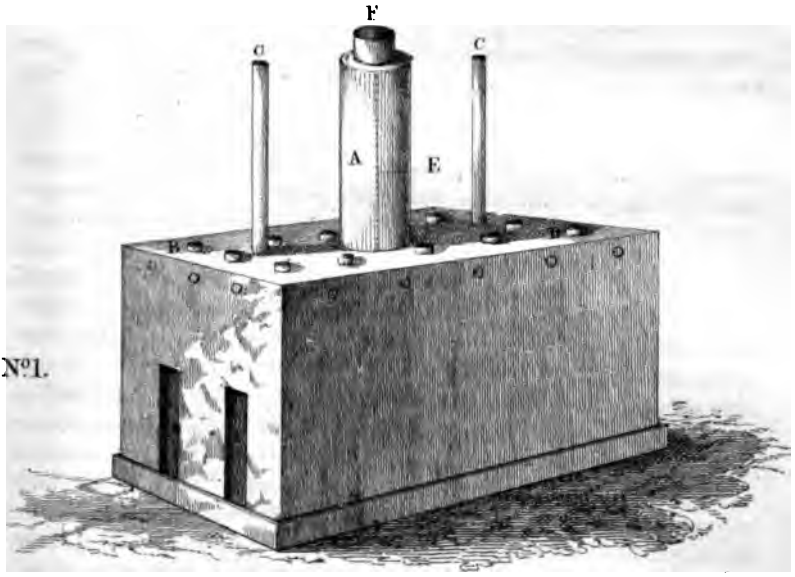
* In this case the egress cylinder acts merely as an air pump, to draw air from the receiver and discharge it into the locality where it is wanted, and the valves required for this purpose might be ordinary self-acting pump-valves. A similar remark applies to the action of the ingress cylinder in the use of the apparatus for producing a cooling effect on the air transmitted, which will then be that of a compressing air-pump to force air from the atmosphere into the receiver. But in order that the same apparatus may be used for the double purpose of heating or cooling, as may be required at different seasons, it will be convenient to have the valves of each cylinder worked mechanically, like those of a steam engine.

ratio of compression to the whole stroke in the egress cylinder would be $\frac{15}{100}$. If 1 lb. of air (or about 15½ cubic feet, at the stated temperature of 80°, and the mean atmospheric pressure,) be to be delivered per second, the motive power required for working the machine would be ·283 of a horse power, were the action perfect, with no loss of effect, by friction, by loss of expansive power due to cooling in the ingress cylinder, or otherwise. If each cylinder be four feet in stroke, and 26·3 inches diameter, the pistons would have to be worked at 30 double strokes per minute.

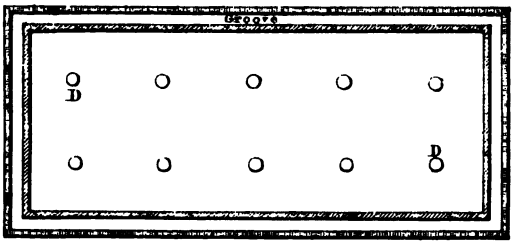
On the other hand, if it be desired to cool air, either the ingress piston must be worked faster than the other, or the stroke of the other must be diminished, or the ingress cylinder must be larger, or an auxiliary ingress cylinder must be added. The last plan appears to be undoubtedly the best, as it will allow the two principal pistons to be worked stroke for stroke together, and consequently to be carried by one piston rod, or by a simple lever, without the necessity of any variable connecting gearing, whether the machine be used for heating or for cooling air; all that is necessary to adapt it to the latter purpose, besides altering the valve gearing, being to connect a small auxiliary piston to work beside the principal ingress cylinder, with which it is to have free communication at each end. If it were required to cool air from 80° to 50° Fahr., the auxiliary cylinder would be required to have its volume $\frac{1}{17}$ of that of each of the principal cylinders; and, if its stroke be the same, its diameter would therefore be a little less than a quarter of theirs. The valves would have to be altered to give compression in the ingress cylinder during the same fraction of the stroke as is required for expansion when the air is heated through the same range of temperature, and the valves of the egress cylinder would have to give the same proportion of expansion as is given of compression in the other case; and the pressure kept up in the receiver, by the action of the pistons thus arranged, would be $1\frac{8}{35}$ atoms, or about 3·2 lbs. on the square inch above the atmospheric pressure. The principal cylinders being of the same dimensions as those assumed above, and the quantity of air required being the same (1 lb. per second), the pistons would have to be worked at only 24·6 double strokes per minute instead of 30, and the horse power required would be ·288, instead of as formerly ·283, when the same machine was used for giving a supply of heated air.

Mr. John Ure then exhibited a Model Ventilating Apparatus which had been constructed by him. He explained its structure and illustrated its action, &c. It consisted of an oblong case, about 2 feet long, 1 foot broad, and 15 inches high, with sides and top, but no fixed bottom. The interior of this case represented that of an apartment to be ventilated. The sides and ends were glazed, so as to enable the observer to ascertain the changes which might occur within. The case (see drawing, No. 1) was provided with a moveable bottom, around the margin of which was a gutter or groove filled with water (see drawing, No. 2), into which the





N°1.



N°2.

Mr URE'S APPARATUS
for illustrating the principle of Ventilation.

walls of the case dipped, thus rendering the case air-tight at its lower part. In the centre of the roof there was a tube *nine inches high* (A) by three and a-half in diameter. At the sides of the roof, and at its ends, were a series of additional tubular openings, each three-fourths of an inch in diameter—in all, eleven in number (B). These projected above the top *one-half inch*, so as to admit of an air-tight cap being put on each. Two other tubes (C), equal in height to the centre one, were placed at either end. These were one inch in diameter, and their use is noticed in the sequel. The bottom of the case was perforated by a number of tubes (D); these rising to the height of three inches in the interior of the case. They could be opened or closed as desired. These openings when patent permitted of the ventilation of the case on the most natural, and consequently the most efficient principle. Practical difficulties, however, are in the way of the universal adoption of this system. The apparatus designed by Mr. Ure had in view ventilation by the roof alone, or in conjunction with the upper margins of walls.

The subjects of experiment and points of comparison lay between the two sorts of tubes first mentioned—the large central aperture and the series of small ones. The case was lifted out of the water channel, and six lamps placed on the case bottom—two at and equally apart from the sides and near to each end, and two in the centre of length. At this period the lamps were burning full blaze. The case was now restored to its place with a diaphragm or division (E), of the exact width, and extending the whole length of the centre tube, thus dividing the tube into two semi-circular vents. By this arrangement, as has long been known, two opposite currents were caused in the tube —one on each side of the diaphragm; the cold air passing in by the one opening, while the heated air and products of combustion made their escape by the other. In less than three minutes a great decay of the energy of combustion in the lamps was quite perceptible, and in about four minutes all the end lamps were much reduced in flame, and two of them were nearly extinguished. The two in the centre, being those immediately under the ventilating arrangement, continued to burn more freely, but still languidly. At this stage the combustion was a true maximum, and remained stationary. Its energy was indicated by the whole amount and local distribution of ventilation which the apparatus as used was capable of supplying. The mode of ventilation here experimented on has been lately patented, and is characterised by and distinguished from that of Mr. Ure by one or more tubes placed in the roof, and each equally divided by a diaphragm, the requisite supply of cold, and the emission of heated and contaminated air, being expected to take place by these semi-tubes.

When the stationary point of ventilation by the model apparatus in use had been reached, the eleven small openings distributed over the top were quickly uncapped, the diaphragm withdrawn, and an amount of the opening of the large centre tube, as nearly equal in area as

possible to the sum of the areas of the eleven small ones, was instantly closed up by means of a cap with an opening in the centre, two inches in diameter (F). The absolute amount of space for charge and discharge was thus left equal to that in the first experiment with the single divided tube. In about 90 seconds the combustion of the lamps was restored to their original brilliancy. The dispersed short tubes supplied the cold air, the heated air making a strong escape by the opening left in the large tube. The entire area of the large opening being about 9·6 square inches; the sum of the areas of the eleven small ones about 6·5, and the area of the discharge tube in this experiment about 3·1, the sum of these two last are equal to the area of the first or large tube. This last experiment was further continued by closing in succession several of the small tubes so as to reduce the energy of combustion to the diminished condition attained and rested at in the first experiment with the single divided tube, being the maximum referred to, as determined, in the first experiment shown. The number of tubes ultimately closed amounted to four, leaving only *seven* open, when a condition sensibly better than the stationary state then referred to, was in three minutes arrived at. At this stage the combustion was not only less languid, but continued more uniformly distributed. According to this result the sum of the areas of the seven tubes for the descending current being about 4·1 square inches, and the area of the descending tube 3·1, together 7·2, the ventilation by Mr. Ure's method of the dispersed tubes was as effective as that in the first experiment (9·6 square inches), *with less than three-fourths of roof opening, while from the uniformity of the rate of combustion in all the lamps, the ventilation was more equably distributed.* A third experiment was made, in which the seven entrance tubes remained as before, but instead of the two-inch escape tube, two tubes, each one inch in diameter (C), situated at different parts of the roof, were opened. The result was that the combustion was still as perfect as in the first experiment. The areas of the entrance tubes being as before 4·1, and the areas of discharge tubes 1·56, in all 5·66 square inches for charge and discharge, the dissimilarity in necessary roof opening came to be 9·6, as compared with 5·66, or little more than *one-half.* The cost for the erecting on the large scale of the two arrangements, when calculated, was found to be in favour of Mr. Ure's proposed method. Other experiments bearing on the certainty and direction of the currents, were successfully exhibited to the meeting.

Mr. Ure, in conclusion, considered that distinct tubes, of unequal lengths, should be employed in ventilation, the taller for the ascending current, and the shorter for the descending; that the tubes, particularly the smaller ones, so as not to interfere with the uniform appearance or elegance of buildings, might be distributed around the upper walls, or on the roofs of the apartments. Those for the ascending current might consist of one centrally placed or several judiciously disposed at intervals along the roof, or otherwise, according to convenience or necessity.

December 15, 1852.—*The* PRESIDENT *in the Chair.*

THE following were admitted as members, viz.:—Thomas Anderson, Mr. James Young, Mr. Edward Meldrum, Mr. William Nielson.

The following were elected as members, viz.:—Mr. John L. Dunn, Mr. Robert Mackay, Mr. William Boyd, Mr. William Broom, Mr. Andrew Jackson, Mr. Edward Meikleham.

Letters were received from the Secretaries of the Royal Society of London, and the Geological Society of London, acknowledging receipt of the last part of the Society's printed Proceedings.

Mr. Ramsay presented a copy of Lecture, by Dr. Lyon Playfair, on Industrial Instruction on the Continent; and of Paper by Professor A. C. Ramsay, on the Superficial Accumulation and Surface Markings of North Wales.

Mr. Bryce read a paper "On the Connection between the Slate Rocks of the South of Scotland and those of the opposite Coast of Ireland."

January 5, 1853.—*The* PRESIDENT *in the Chair.*

MR. JOHN L. DUNN, Mr. Robert Mackay, Mr. William Boyd, Mr. William Broom, Mr. Andrew Jackson, jun., and Mr. Edward Meikleham, were admitted members.

Dr. Thomas H. Rowney, College Laboratory, was elected a member.

The following were proposed as members, viz.:—Mr. George Donaldson, Mr. James Taylor.

A letter was received from the Secretaries of the Literary and Philosophical Society of Manchester, acknowledging receipt of the last part of the Society's printed Proceedings.

Mr. W. J. Macquorn Rankine moved that the Society memorialise the Lords of Her Majesty's Treasury, in favour of employing the scale of six inches to a mile, in preparing the maps of the Ordnance Survey in the Counties of Lanark, Ayr, and Renfrew, and also recommending that the levels of the ground be marked by figures at the more important points, and by contour lines. Mr. Liddell seconded the motion, which was agreed to; and a committee, consisting of Professor William Thomson, Mr. Liddell, Mr. Bryce, Mr. Reid, and Mr. Rankine, was appointed to prepare and transmit the memorial, and also to communicate on the subject with the Provosts of burghs, and the Clerks to the Commissioners of Supply, in the Western Counties of Scotland.

Mr. William Johnson read a paper "On the Patent Laws and their recent changes."

Mr. Macquorn Rankine read a paper "On the General Law of the Transformation of Energy." The discussion on the latter paper was adjourned till next meeting.

XXXIV.—*On the General Law of the Transformation of Energy.*

By W. J. MACQUORN RANKINE.

ACTUAL, or SENSIBLE ENERGY, is a measurable, transmissible, and transformable condition, whose presence causes a substance to tend to change its state in one or more respects. By the occurrence of such changes, actual energy disappears, and is replaced by

POTENTIAL or LATENT ENERGY; which is measured by the product of a change of state into the resistance against which that change is made.

(The *vis viva* of matter in motion, thermometric heat, radiant heat, light, chemical action, and electric currents, are forms of actual energy; amongst those of potential energy are the mechanical powers of gravitation, elasticity, chemical affinity, statical electricity, and magnetism.)

The law of the *Conservation of Energy* is already known, viz.:—that the sum of all the energies of the universe, actual and potential, is unchangeable.

The object of the present paper is to investigate the law according to which all *transformations of energy*, between the actual and potential forms, take place.

Let V be the magnitude of a measurable state of a substance;

U, the species of potential energy which is developed when the state V increases;

P, the common magnitude of the tendency of the state V to increase, and of the equal and opposite resistance against which it increases; so that—

$$dU = P\,dV; \text{ and } P = \frac{dU}{dV} \quad \ldots\ldots \quad (A.)$$

Let Q be the quantity which the substance possesses, of a species of actual energy whose presence produces a tendency of the state V to increase.

It is required to find how much energy is transformed from the actual form Q to the potential form U, during the increment dV; that is to say, the magnitude of the portion of dU, the potential energy developed, which is due to the disappearance of an equivalent portion of actual energy of the species Q.

The development of this portion of potential energy is the immediate effect of the presence in the substance of the total quantity Q of actual energy.

Let this quantity be conceived to be divided into indefinitely small equal parts dQ. As those parts are not only equal, but altogether alike in nature and similarly circumstanced, their effects must be equal; therefore, the effect of the total energy Q must be equal simply to the effect of one of its small parts dQ, multiplied by the ratio $\frac{Q}{dQ}$.

But the effect of the indefinitely small part dQ, in causing development of potential energy of the species U, during the increment of state dV, is represented by—

$$\frac{dP}{dQ}dQdV$$

whence it follows, that the effect of the presence of the total actual energy Q, in causing transformation of energy from the actual form Q to the potential form U, is expressed by the following formula:—

$$Q.\frac{dP}{dQ}.dV \quad \ldots\ldots\ldots\ldots(1.)$$

which is the solution required, and is the symbolical expression of the GENERAL LAW OF THE TRANSFORMATION OF ENERGY:—

The effect of the whole Actual Energy present in a substance, in causing Transformation of Energy, is the sum of the effects of all its parts.

The difference between this quantity and the potential energy developed, viz.:—

$$\left(P—Q.\frac{dP}{dQ}\right)dV,$$

represents a portion of potential energy, due to causes different from the actual energy Q. This difference is null, when the resistance $\left(P=\frac{dU}{dV}\right)$ against which the state V increases, is simply proportional to the total actual energy Q.

It is next proposed to find the quantity of actual energy of the form Q, which must be transmitted to the substance from without, in order that its total actual energy may receive the increment dQ, and its state V at the same time, the increment dV.

This quantity is composed of three parts, viz:—actual energy, which preserves its form, dQ; actual energy which transforms itself to some unknown form, in consequence of the resistance which is offered to the increase of the total actual energy, LdQ; actual energy, already determined, which transforms itself into potential energy of the form U, $Q.\frac{P}{dQ}.dV$; the sum of these parts being—

$$d\cdot Q = (1 + L)\,dQ + Q.\frac{dP}{dQ}.dV \quad \ldots\ldots\ldots\ldots(2.)$$

in which nothing remains to be determined except the function L.

If we subtract from the above formula, the total potential energy developed during the increment dV, viz:—

$$P.dV,$$

we obtain the algebraical sum of the energies, actual and potential.

received and developed by the substance during the changes dQ, dV; which is thus expressed:—

$$d\Psi = d\cdot Q - d\cdot U = (1 + L)\, dQ + \left(Q \frac{d}{dQ} - 1\right) P.dV \quad \ldots\ldots (B.)$$

This quantity must be the exact differential of a function of Q and V; for otherwise it would be possible, by varying the order of the increments dQ, dV, to change the sum of the energies of the universe.

It follows that—

$$\frac{dL}{dV} = \frac{d}{dQ}\left(Q \frac{d}{dQ} - \right) P = Q. \frac{d^2}{dQ^2} P$$

and consequently, that

$$L = f'(Q) + Q. \frac{d^2}{dQ^2} \int P dV$$

where $f'(Q)$ is a function of Q and constants, the first derivative of $f'(Q)$.

We find at length the following equation—

$$d\Psi = d.Q - d.U = \left(1 + f'(Q) + Q.\frac{d^2}{dQ^2} \int P dV\right) dQ + \left(Q \frac{d}{dQ} - 1\right)$$

$$= d. \left\{Q + f(Q) + \left(Q \frac{d}{dQ} - 1\right) \int P dV\right\} \quad \ldots (3.)$$

which represents the algebraical sum of the energy, actual and potential, received and developed by a substance, when the total actual energy of the species Q, and the state V, receive respectively the increments dQ, dV.

It is to be observed, that in the last equation, the symbol $\int P.dV$ denotes a *partial integral*, taken in treating the particular value of Q, to which it corresponds as a constant quantity; while d. U represents the real magnitude of the potential energy developed.

The application of the general law of the transformation of energy may be extended to any number of kinds of energy, actual and potential, by means of the following equation: $d.\Psi = \Sigma d.Q - \Sigma d.U$.

$$= \Sigma \left\{\left(1 + f'(Q) + Q\cdot\Sigma \frac{d^2}{dQ^2} \int P dV\right) dQ\right\} + \Sigma \left\{\left(\Sigma Q \frac{d}{dQ} - 1\right) P dV\right\}$$

$$= d \left\{\Sigma Q + \Sigma f(Q) + \Sigma \left(\Sigma\cdot Q \frac{d}{dQ} - 1\right) \int P dV\right\} \quad \ldots\ldots (4.)$$

This equation is the complete expression of the general law of the transformation of energy of all possible kinds, known and unknown. It affords the means, so soon as the necessary experimental data have been obtained, of analysing every development of potential energy, and referring its several portions to the species of actual energy from which they have been produced.

Amongst the consequences of this law, the author deduces that which may be called the general principle of the maximum effect of engines.

An engine consists essentially in a substance, whose changes of state, and of actual energy, between given limits, are so regulated as to produce a permanent transformation of energy.

Let Q_1 be the given superior limit of actual energy; Q_2, the inferior limit.

To produce the maximum permanent transformation of energy from the actual to the potential form, the substance must undergo a cycle of four operations, viz.:—

First operation.

The substance, preserving the constant quantity Q_1 of actual energy, passes from the state V_A to the state V_B, receiving from without the following quantity of actual energy, which is converted into potential energy:—

$$H_1 = Q_1 \cdot \frac{d}{dQ} \int_{V_A}^{V_B} P.dV$$

Second operation.

The substance passes from the superior limit of actual energy Q_1, to the inferior limit Q_2. Let V_C be the value of the state V at the end of this operation.

Third operation.

The substance preserving the constant quantity Q_2 of actual energy, passes from the state V_C to the state V_D, transmitting to external substances the following quantity of actual energy, produced by the disappearance of potential energy:—

$$H_2 = Q_2 \frac{d}{dQ} \int_{V_D}^{V_C} PdV$$

Fourth operation.

The substance is brought back to its original actual energy Q_1, and state V_A, thus completing the cycle of operations.

In order that the second and fourth operations may be performed without expenditure of energy, the following condition must be fulfilled:—

$$\frac{d}{dQ} \int_{V_D}^{V_C} PdV \text{ (for } Q = Q_2) = \frac{d}{dQ} \int_{V_A}^{V_B} PdV \text{ (for } Q = Q_1.)$$

This being the case, the total expenditure of energy during a cycle of operations will be H^1, being the quantity converted from the actual to the potential form during the first operation; the energy lost will be H_2, the quantity reconverted to the actual form, and transmitted to external substances, during the third operation; and the quantity of energy per-

manently transformed from the actual to the potential form, that is to say, the work done by the engine will be—

$$H_1 - H_2 = (Q_1 - Q_2) \frac{d}{dQ} \int_{V_A}^{V_B} P dV \text{ (for } Q = Q_1) \ldots\ldots (6.)$$

The ratio of this work to the total expenditure of energy is

$$\frac{H_1 - H_2}{H_1} = \frac{Q_2 - Q_2}{Q_1} \quad \ldots\ldots\ldots (7.)$$

This principle is applicable to all possible engines, known and unknown.

In the sequel of the paper, the author gives some examples of the application of the general principles of the transformation of energy to the theory of heat, and to that of electro-magnetism; and deduces from them, as particular cases, several laws already known through specific researches.

The details of the application of these principles to the theory of heat are contained in the sixth section of a memoir read to the Royal Society of Edinburgh, "On the Mechanical Action of Heat."

The actual energy produced by an electric pile in unity of time is expressed by—

$$Q = Mu$$

where M is the electro-motive force, and u, the strength of the current.

The actual energy of an electric circuit is expressed by—

$$Ru^2$$

where R is the resistance of the circuit. This energy is immediately and totally transformed into sensible heat.

The proportion of the actual energy produced in the pile which is transformed into mechanical work by an electro-dynamic machine is represented by—

$$\frac{Q_1 - Q_2}{Q_2} = \frac{M - Ru}{M}$$

The strength of the current is known to be found by means of the equation—

$$u = \frac{M - N}{R}$$

where N is the negative or inverse electro-motive force of the apparatus by means of which electricity is transformed into mechanical work. Hence

$$\frac{Q_1 - Q_2}{Q_1} = \frac{N}{M}$$

The above particular forms of the general equation, agree with formulæ already deduced from special researches by Mr. Joule and Professor William Thomson.

January 19, 1853.—*The* PRESIDENT *in the Chair.*

MR. GEORGE DONALDSON and Mr. James Taylor were elected members.

The discussion on Mr. Rankine's paper "On the General Law of the Transformation of Energy," was resumed, and Professor William Thomson and Mr. James Reid expressed their opinion on the subject.

A paper "On the Mechanical Values of Distributions of Electricity, Magnetism, and Galvanism," and another "On Transient Electric Currents," were then read by Professor William Thomson.

XXXV.—*On the Mechanical Values of Distributions of Electricity, Magnetism, and Galvanism.* By PROFESSOR W. THOMSON.

I. ELECTRICITY AT REST.

To electrify an insulated conductor (a Leyden phial, for instance, or any mass of metal resting on supports of glass,) in the ordinary way by means of an electrical machine, requires the expenditure of work in turning the machine. But inasmuch as part, obviously by far the greater part, of the work done in this operation goes to generate heat by means of friction, and of the small residue some, it may be a considerable proportion, is wasted in generating heat (electrical light being included in the term) by the flashes, illuminated points, and sparks, which accompany the transmission of the electricity from the glass of the machine where it is first excited, to the conductor which receives it, the mechanical value of the electrification thus effected would be enormously overestimated if it were regarded as equivalent to the work that has been spent. Notwithstanding, the mechanical value of any electrification of a conductor has a perfectly definite character, and may be calculated with ease in any particular case, by means of formulæ demonstrated in this communication. The simplest case is that of a single conductor insulated at a distance from other conductors, or with only uninsulated conducting matter in its neighbourhood. In this case the mechanical value of the electrification of the conductor, is equal to *half the square of the quantity of electricity, multiplied by the* capacity of the conductor.*

In any case whatever, the total mechanical value of all the distributions of electricity on any number of separate insulated conductors electrified with any quantities of electricity, is demonstrated by the author to be equal to half the sum of the products obtained by multiplying the "potential"† in each conductor by the quantity of electricity by which

* A term introduced by the author to signify the proportion of the quantity of electricity that the conductor would retain to that which it would communicate to a conducting ball of unit radius, insulated at a great distance from other conducting matter, if connected with it by means of a fine wire.

† A term first introduced by Green, which may be defined as the quantity of mechanical work that would have to be spent to bring a unit of electricity from a great distance up to the surface of the conductor, supposed to retain its distribution unaltered.

it is charged. Each term of this expression does not represent the independent value of the actual distribution on the conductor to which it corresponds, inasmuch as the "potential" in each depends on the presence of the others, when they are near enough to exert any sensible mutual influence; but independent expressions of these independent values are readily obtained, although not in a form convenient for statement here; and the author proves that their sum is equal to the total value, as calculated by the preceding expression. When a conductor is discharged without other mechanically valuable effects being developed, the heat generally, as for instance in the sparks produced when the knob of a Leyden phial is put in communication with the outside coating, or when a flash of lightning takes place, is equal in mechanical value to the distribution of electricity lost. Hence, by what precedes the amount of heat is proportional to the *square* of the quantities discharged, as was first demonstrated by Joule, in a communication to the Royal Society in 1840, although it had been announced by Sir W. Snow Harris as an experimental result, to be simply proportional to the quantity. Mr. Joule's result has been verified by independent experimenters in France, Italy, and Germany. The author pointed out other applications of his investigation, some of a practical kind, and others in the Mathematical Theory of Electricity. He mentioned, that although he had first arrived at the results in 1845, and used them in papers published in that year, the first explicit publication of the theorem regarding the mechanical value of the electrification of a conductor appears to be in 1847, in a paper entitled "Ueber die Erhaltung der Kraft," by Helmholz.

II. MAGNETISM.

If a piece of soft iron be allowed to approach a magnet very slowly from a distant position, and be afterwards drawn away so rapidly that at the instant when it reaches its primitive position, where it is left at rest, it retains as yet sensibly unimpaired the magnetization it had acquired at the nearest position, a certain amount of work must have been finally expended on the motion of the iron. For during the approach, the iron has only the magnetization due to the action of the magnet on it in its actual position at each instant, but at each instant of the time in which the iron is being drawn away, it has the whole magnetization due to the action of the magnet on it when it was at the nearest. Hence it is drawn away against more powerful forces of attraction by the magnet, than those with which the magnet attracts it during its approach; from which it follows that more work is spent in drawing the iron away than had been gained in letting it approach the magnet. The sole effect due to this excess of work is the magnetization which the iron carries away with it; and consequently, the mechanical value of this magnetization must be precisely equal to the mechanical value of the balance of work spent in producing it.

After a very short time has elapsed with the piece of soft iron at a great distance from the magnet, it will have lost, as is well known, all or nearly all the magnetization which it had acquired temporarily in the neighbourhood of the magnet; and in this short time some energy, equivalent to that of the magnetization lost, must have been produced. Mr. Joule's experiments show that this energy consists of heat, which is generated during the demagnetization of the iron; and we infer the remarkable conclusion, that at the end of the process, which has been described, or of any motion of a piece of soft iron in the neighbourhood of a magnet, from a certain position and back to the same, the iron will be as much the warmer than it was at the beginning, as it would have been without any magnetic action, if it had received the heat that would be generated by the expenditure of the same amount of work on mere friction.

The same considerations are applicable to the magnetization of a piece of steel, with this difference, that according to the hardness of the steel, the magnetization which it receives in the nearest position will be more or less permanent, and if there be any demagnetization after removal from the magnet, it will be much less complete than in the case of soft iron, and that heat will be necessarily generated both during the magnetization which takes place during the gradual approach, and in the subsequent demagnetization. Further, by putting together a number of pieces of steel, each separately magnetized, a complete magnet will be formed, of which the mechanical value will be equal to the sum of the mechanical values of its parts, increased or diminished by the amount of work spent or gained in bringing them together.

Upon the principles which have been explained, the author has investigated the mechanical value of any conceivable distribution of magnetism, in any kind of substance. The result, which cannot be well expressed, except in mathematical language, is as follows:—

$$\iiint \lambda q^2 dxdydz + \frac{1}{8\pi}\int_{\infty}^{\infty}\iiint \mathrm{R}^2 dzdydz$$

where R denotes the resultant magnetic force at any internal or external point (x, y, z), of the intensity of magnetization at a point (x, y, z), of the magnet, and λ a quantity depending on the nature of the substance at this point.

The integral constituting the first term of this expression, includes the whole of the magnetized substance, and expresses the sum of the separate mechanical values of the distributions in all the parts obtained by infinitely minute division along the lines of magnetization. The second term expresses the amount of work that would have to be spent to put these parts together, were they given separately, each with the exact magnetization that it is to have when in its place in the whole. If the substance be perfectly free in its susceptibility for magnetization or demagnetization,

λ will express such a function of the inductive capacity that if a ball of similar substance be placed in a magnetic field where the force is F, the intensity of the magnetization induced in it will be—

$$\frac{F}{2\lambda+\frac{1}{3}\tau}$$

III. ELECTRICITY IN MOTION.

If an electric current be excited in a conductor, and then left without electro-motive force, it retains energy to produce heat, light, and other kinds of mechanical effect, and it lasts with diminishing, or it may be with alternately diminishing and increasing strength: before it finally ceases an electrical equilibrium is established, as is amply demonstrated by the experiments of Faraday and Henry, on the spark which takes place when a galvanic circuit is opened at any point, and by those of Weber, Helmholz, and others on the electro-magnetic effects of varying currents. The object of the present communication is to show how the mechanical value of all the effects that a current in a close circuit can produce after the electro-motive force ceases, by a determination, founded on the known laws of electro-dynamic induction, of the mechanical value of the energy of a current of given strength, circulating in a linear conductor (a bent wire, for instance) of any form. To do this, in the first place it may be remarked, that although a current, once instituted in a conductor, will circulate in it with diminishing strength after the electro-motive force ceases, just as if the electricity had inertia, and will diminish in strength according to the same, or nearly the same, laws as a current of water or other fluid, once set in motion and left without moving force, in a pipe forming a closed circuit. But according to Faraday, who found that an electric circuit consisting of a wire doubled on itself, with the two parts close together, gives no sensible spark when suddenly opened, compared to that given by an equal length of wire bent into a coil, it appears that the effects of ordinary *inertia* either do not exist for electricity in motion, or are but small compared with those which, in a suitable arrangement, are produced by the "induction of the current upon itself." In the present state of science it is only these effects that can be determined by a mathematical investigation; but the effects of electrical inertia, should it be found to exist, will be taken into account by adding a term of determinate form to the fully determined result of the present investigation which expresses the mechanical value of a current in a linear conductor, as far as it depends on the induction of the current on itself.

The general principle of the investigation is this; that if two conductors, with a current sustained in each by a constant electro-motive force, be slowly moved towards one another, and there be a certain *gain of work* on the whole, by electro-dynamic force, operating during the motion, there will be twice as much as this of work spent by the electro-motive

forces (for instance, twice the equivalent of chemical action in the batteries, should the electro-motive forces be chemical,) over and above that which they would have had to spend in the same time if the conductors had been at rest merely to keep up the currents, because the electro-dynamic induction produced by the motion will augment the currents; while on the other hand, if the motion be such as to require the *expenditure* of work against electro-dynamic forces to produce it, there will be twice as much work saved off the action of the electro-motive forces by currents being diminished during the motion. Hence the aggregate mechanical value of the currents in the two conductors, when brought to rest will be increased in the one case by an amount equal to the work done by mutual electro-dynamic forces in the motion, and will be diminished by the corresponding amount in the other case. The same considerations are applicable to relative motions of two portions of the same linear conductor (supposed perfectly flexible). Hence it is concluded that the mechanical value of a current of given strength in a linear conductor of any form, is determined by calculating the amount of work against electro-dynamic forces, required to double it upon itself, while a current of constant strength is sustained in it. The mathematical problem thus presented leads to an expression for the required mechanical value consisting of two factors, of which one is determined according to the form and dimensions of the line of the conductor in any case, irrespectively of its section, and the other is the square of the strength of the current. If it be found necessary to take inertia into account, it will be necessary to add to this expression a term consisting of two factors, of which one is directly proportional to the length of the conductor, and inversely proportional to the area of its section, and the other is the square of the strength of the current, to obtain the complete mechanical value of the electrical motion.

XXXVI.—*On Transient Electric Currents.* By PROF. WM. THOMSON.

THE object of this communication is to determine the motion of electricity at any instant after an electrified conductor of given capacity, is put in connection with the earth by means of a wire or other linear conductor of given form and given resisting power. The solution is founded on the *equation of energy* (corresponding precisely to "the equation of vis-viva" in ordinary dynamics) which is sufficient for the solution of every mechanical problem, involving only one variable element to be determined in terms of the time. That there is only one such variable in the present case follows from two assumptions which are made regarding the data, namely,

(1.) That the electrical capacity of the first mentioned, or principal conductor, as it will be called, is so great in comparison with that of the second or discharger, as to allow no appreciable proportion of its original charge to be contained in the discharger at any instant of the discharge,

which will imply that the strength of the current at each instant must be sensibly uniform through the whole length of the discharger.

(2.) That there is no sensible resistance to conduction over the principal conductor, so that the amount of charge left in it at any instant of the discharge will be distributed on it in sensibly the same way as if there was complete electrical equilibrium.

The theorems demonstrated in the first and third parts of the previous communication give expressions for the mechanical values of the charge left in the principal conductor, and the electrical motion in the discharger, at any instant, in terms of the amount of that charge, and the rate at which it is diminishing. The sum of these two quantities, constitutes the whole electro-statical and electro-dynamical energy in the apparatus, and the diminution which it experiences in any time, must be mechanically compensated by heat generated in the same time. We have thus an equation between the diminution of the electrical energy in any infinitely small time, and the expression according to Joule's law for the heat generated in the same time in the discharger multiplied by the mechanical equivalent of the thermal unit. The equation so obtained is in the form of a well-known differential equation, of which the integral gives the quantity of electricity left at any instant in the principal conductor, and consequently expresses the complete solution of the problem. Precisely the same equation and solution are applicable to the circumstances of a pendulum, drawn through a small angle from the vertical, and let go in a viscous fluid, which exercises a resistance simply proportional to the velocity of the body moving through it.

The interpretation of the solution indicates two kinds of discharge, presenting very remarkable distinguishing characteristics; a continued discharge, and an oscillatory discharge; one or other of which will take place in any particular case. In the continued discharge the quantity of electricity on the principal conductor diminishes continuously, and the discharging current first increases to a maximum, and then diminishes continuously until after an infinite time equilibrium is established. In the oscillatory discharge, the principal conductor first loses its charge, becomes charged with a less amount of the contrary kind of electricity, becomes again discharged, and again charged with a still smaller amount of electricity, but of the same kind as the initial charge, and so on for an infinite number of times, until equilibrium is established; the strength of the current and its direction, in the discharger, has corresponding variations; and the instants when the charge of either kind of electricity on the principal conductor is at the greatest, being also those where the current in the discharger is on the turn, follow one another at equal intervals of time. The continued or the oscillatory discharge takes place in any particular case, according to the electrical capacity of the principal conductor, the electro-dynamical capacity of the discharger, and the resistance of the discharger to the conduction of electricity. Thus, if the discharger be given, it will effect a continued or an oscillatory discharge, ac-

cording as the capacity of the principal conductor exceeds or falls short of a certain limit. If the principal conductor, and the length and substance of the discharger, be given, the discharge will be continued or oscillatory according as the electrodynamic capacity of the latter, depending as it does on the form into which it is bent, falls short of, or exceeds a certain limit. Lastly, if the principal conductor, and the length and form of the discharger be given, the discharge will be continued or oscillatory, according as the resistance of the discharger to conduction exceeds or falls short of a certain limit.

It ought to be remarked that, although the electrical equilibrium is not rigorously attained, whatever kind of discharge it may be, in any finite time; yet practically, in all ordinary experimental cases the discharge is finished almost instantaneously as regards all appreciable effects; and the great obstacle in the way of experimenting at all on the subject arises from the difficulty of arranging the circumstances, so that the periods of time indicated by the theory for the succession of various phenomena, (as for instance, the alternations of the charges of the contrary electricity on the principal conductor), may not be inappreciably small.

It is not improbable that double, triple, and quadruple flashes of lightning which are frequently seen on the continent of Europe, and sometimes, though not so frequently, in this country, lasting generally long enough to allow an observer, after his attention is drawn by the first light of the flash, to turn his head round and see distinctly the course of the lightning in the sky, result from the discharge possessing the oscillatory character. A corresponding phenomenon might probably be produced artificially on a small scale, by discharging a Leyden phial or other conductor across a very small space of air, and through a linear conductor of large electro-dynamic capacity and small resistance. Should it be impossible, on account of the too great rapidity of the successive flashes, for the unaided eye to distinguish them, Wheatstone's method of a revolving mirror might be employed, and might show the spark as several points or short lines of light separated by dark intervals, instead of a single point of light, or of an unbroken line of light, as it would be if the discharge were instantaneous, or were continuous and of appreciable duration.

The experiments by Riess and others on the magnetization of fine steel needles by the discharge of electrified conductors, illustrate in a very remarkable manner the oscillatory character of the discharge in certain circumstances; not only when, as in the case with which we are at present occupied, the whole mechanical effect of the discharge is produced within a single linear conductor, but when induced currents in secondary conductors generate a portion of the final thermal equivalent.

The decomposition of water by electricity from an ordinary electrical machine, in which, as has been shown by Faraday, more than the electrochemical equivalent of the whole electricity that passes appears in oxygen and hydrogen rising mixed from each pole, is probably due to electrical

oscillations in the discharger consequent on the successive sparks.* Thus, if the general law of electro-chemical decomposition be applicable to currents of such very short duration as that of each alternation in such an oscillatory discharge as may take place in these circumstances, there will be decomposed altogether as much water as is electro-chemically equivalent to the sum of the quantities of electricity that pass in all the successive currents in the two directions, while the quantities of oxygen and hydrogen which appear at the two electrodes will differ by the quantities arising from the decomposition of a quantity of water electro-chemically equivalent to only the quantity of electricity initially contained by the principal conductor. The mathematical results of the present communication lead to an expression for the quantity of water decomposed by an oscillatory discharge in any case to which they are applicable, and show that the greater the electro-dynamic capacity of the charger, the less its resistance, and the less electro-statical capacity of the principal conductor, the greater will be the quantity of water decomposed. Probably the best arrangement in practice would be one in which merely a small ball or knob is substituted for a principal conductor fulfilling the conditions prescribed above; but those conditions not being fulfilled, the circumstances would not be exactly expressed by the formulæ of the present communication; the resistance would be much diminished, and consequently the whole quantity of water decomposed much increased, by substituting large platinum electrodes for the mere points used by Wollaston; but then the oxygen and hydrogen separated during the first direct current would adhere to the platinum plates and would be in part neutralized by combination with the hydrogen and oxygen brought to the same plates respectively by the succeeding reverse current; and so on through all the alternations of the discharge. In fact, if the electrodes be too large, all the equivalent quantities of the two gases brought successively to the same electrode will recombine, and at the end of the discharge there will be only oxygen at the one electrode and only hydrogen at the other, in quantities electro-chemically equivalent to the initial charge of the principal conductor. Hence we see the necessity of using very minute electrodes, and of making a considerable quantity of electricity pass in each discharge, so that each successive alternation of the current may actually liberate from the electrodes some of the gases which it draws from the water. Probably the most effective arrangement would be one in which a Leyden phial or other body of considerable capacity is put in connection with the machine and discharged in sparks through a powerful discharger, not only of great electro-dynamic capacity, and of as little resistance as possible except where the metallic communication is broken in the electrolytic vessel, but of considerable electro-statical capacity, so that all, or as great a

* This conjecture was first, so far as I am aware, given by Helmholz, the existence of electrical oscillations in many cases of discharge having been indicated by him as a probable conclusion from the experiments of Riess, alluded to in the text.

portion as possible, of the oscillating electricity may remain in it and not give rise to successive sparks across the space of air separating the discharger from the source of the electricity.

The paper is concluded with applications of the results to determine the laws, according to which a current varies at the commencement and end of any period, during which a constant electro-motive force, such as that of a galvanic battery, acts in a conductor of given electro-dynamic capacity and resistance, and to show how the relation between the electrostatical and electro-dynamic units of electrical quantity and electro-motive force may be experimentally determined.

February 2, 1853.—*The* PRESIDENT *in the Chair.*

MR. DONALDSON and Mr. Taylor were admitted as members.

Mr. Malcolm M'Niel Walker, optician, was proposed a member by Mr. James R. Napier, Mr. W. M. Buchanan, and Mr. Charles Griffin.

Papers were read by Mr. James R. Napier, "On the Bulk-heads and Water-tight Compartments of Steam-vessels."

"Experiments on the Evaporation of Water in Copper, Iron, and Lead Vessels."

"Experiments on the Compasses of Iron Vessels."

XXXVII.—*Illustrations of the Utility of Water-tight Compartments in Iron Vessels.* By Mr. J. R. NAPIER.

IRON steam-vessels are obliged by Act of Parliament to have three water-tight compartments, which is usually done by placing one bulkhead before the machinery and one abaft it.

It is well known that many accidents have happened to vessels, which would in all probability have been fatal, or attended with very serious loss, but for the timely assistance of one or other of these compartments.

The "Fire Queen," for instance, a small screw vessel, originally built as a pleasure yacht for Mr. Assheton Smith, was in 1850 placed as a goods and passenger vessel upon the Glasgow, Ardrossan, and Ayr station. One afternoon, at low water, on leaving Ardrossan harbour, she struck the fluke of an anchor a few feet before one of the water-tight bulkheads; the fore compartment filled with water, and the bow sank to the bottom. The middle and after compartments, however, kept her from entirely sinking, as the tide rose they floated her to the shore, where the cargo was discharged without damage, and the hole being temporarily stopped, the vessel steamed to Glasgow next day for repairs.

The late "Metropolitan," screw steamer, also on one of her passages from London to Glasgow, struck a sailing ship in the Bristol Channel. The ship sank in about ten minutes afterwards, and although the foremost compartment of the steamer was filled with water, she proceeded on

her voyage and discharged her cargo at Glasgow as if nothing ha happened. The foremost compartment in this case, however, was ver small, as an additional water-tight bulkhead had been placed near th stern, in order to make a fourth compartment. On a subsequent occasio this steamer was herself struck amidships by a sailing vessel off the sout of England, but she did not sink till about *three hours* after the acciden though the centre compartment filled immediately.

Many other instances might be mentioned; the most remarkable ca with which I am acquainted is that of the "Thistle" steamer, a vess which after striking the rocks on the North of Ireland, steamed witho assistance thence to Greenock, a distance of about seventy nautical mile across the North Channel, with the fore-deck under water, the fore an after compartments filled with water, and nothing but the centre engine compartment free. She arrived in other respects safely at h destination.

The accompanying letter from the managers of the vessel, gives th particulars of the accident, and the sketches taken partly from the ves while repairing, and partly from the original designs and from informati received from those on board during the accident, show very correct the appearance she presented on arriving at Greenock, and also th injuries the bottom sustained upon the rocks.

If it were necessary to lengthen this paper many other examples mig be given, but the preceding show that water-tight bulkheads of sufficie strength have been the means of saving both lives and property.

Care, however, must be taken in endeavouring to make strong bul heads water-tight, not to weaken materially the general strength of th ship by piercing the shell plates with too many holes in a line.

From the descriptions given at the time of the loss of H.M.S. "Birke head" off the south coast of Africa, it appeared that her sudden breakin up must have been owing to this cause.

"Glasgow, 11th March, 1852.

"ROBERT NAPIER, ESQ.

"DEAR SIR,—You have no doubt heard of the accident to 'Thistl steamer, on the evening of Saturday last, while proceeding along th north coast of Ireland in a fog. She struck on some sunken rocks, a stove in part of the bottom plates both forward and aft. The fore-hol after-hold, and cabins filled, but fortunately the middle compartmen forming the engine and boiler space, remained uninjured. And aft she was floated off the rock the bulkheads both before and abaft th engine space stood firm, and she returned to Greenock by the power her own engines alone, without assistance from any other vessel, thou solicited by two steam vessels to allow them to assist. The fact of vessel of her tonnage (670 tons) steaming across the Irish Chann safely with her holds and cabins full of water, the mid compartment the vessel only keeping free, is most remarkable, and a strong testimo to the value of *water-tight bulkheads.*




Fig. 1
Fig

"Those in the 'Thistle' were made particularly strong; and we think it must give you gratification as the builder of the vessel, to learn it is to that circumstance (the strength of her bulkheads) that the safety of the vessel and the passengers on board of her is to be attributed.

"Captain Dalzell, agent of the Glasgow underwriters, happened to be on board the time of the accident, he was the only passenger who ventured to return with her, and he tells us that but for her strong substantial bulkheads, the vessel must have gone down, and been another case similar to the 'Orion.' The case altogether deserves the notice of both shipowners and shipbuilders.

"Yours truly,

(Signed) "THOS. CAMERON & Co."

XXXVIII.—*Experiments on the Evaporation of Water in Copper, Iron, and Lead Vessels.* By Mr. J. R. NAPIER.

IN many works on the steam engine it is stated that copper, on account of being a better conductor of heat than iron, is therefore a better material for steam boilers.

In the last edition of the "Encyclopædia Brittanica," for example, Mr. John Scott Russell affirms that "Copper is the best of all substances for steam engine boilers in a mechanical point of view; that it is not the best in a mercantile point of view," he says, "is proved by the almost universal use of wrought iron boilers. Yet it is difficult to see why this should be the case, when we remember that copper lasts for ever, is worth when old nearly two-thirds of its first cost, besides being a much *better conductor of heat*, and so saving *fuel* and *space*. The efficiency of a copper boiler," he adds, "in generating steam is to that of iron as three to two."

Mr. Damens, in his paper on Boilers in the Appendix to Tredgold on the Steam Engine, states, "I confidently express my opinion that copper boilers are very far superior to iron for marine purposes." And he gives as one of his reasons "that copper is a better conductor of heat than iron, and that if the advantages of encasing marine boilers with non-conducting matter were properly considered and availed of, boilers might be reduced in capacity, weight, and expense, to the obvious account of the proprietor and to the increased stowage of fuel."

Prideaux also in his rudimentary treatise on Fuel, published this year, states "That as the conducting power of copper is to that of iron as 2·4 to 1, a much smaller extent of heating surface and water space suffice."

Mr. Scott Russell might either have removed his difficulties by a simple experiment, or have pretty safely concluded, from the almost universal adoption of iron boilers, even for the longest voyages, where

if there really were any saving of space and fuel, this would not be the case, that the efficiency of 3 to 2 was altogether an error.

In order to satisfy myself, and perhaps others also, whether the quantity of water evaporated followed the law of the conducting power of the substances in which it was evaporated, (the foregoing quotations being evidently based on this supposition,) I had a number of vessels made of copper, iron, and lead, and the time which equal quantities of water took to evaporate noted, and as if the preceding statements were true, I certainly expected to find that the evaporation would be completed in much less time in a thin copper vessel than in a thick lead one. I was satisfied that the mode I adopted of conducting the experiments, though rude, was sufficiently accurate to get at least a rough idea of the subject. The vessels, five inches diameter and two and a-half inches deep, were placed over a gas-burner, first with wire gauze between, and latterly without it, and the water evaporated to dryness. The results are as follows:—

Quantity of water boiled off.	Iron vessel $\frac{1}{30}$ inch thick.		Copper vessel $\frac{1}{30}$ inch thick.		Iron sides $\frac{1}{30}$, & cop. bot. $\frac{1}{30}$.		Iron sides $\frac{1}{30}$, & cop. bot. ¼.		Iron sides $\frac{1}{30}$, lead bottom ¼.	
Fluid. oz.	Min.	Sec.	Min.	Sec.	Min.	Sec.	Min.	Sec.	Min.	Sec.
4	19	...	18	30	...	...	...	...	...	...
11	33	...	30	45	...	...	...	...	...	...
5½	50	...	44	...	...	...	...	...	...	...
4	35	40	...	...	36	50	...	...	...	...
	...	...	...	...	53	30	54	...	55	...

I think from this it is evident that the conducting power of the continuous metal has little or nothing to do with the quantity of water evaporated in a given time, any more than the thickness of the metal has to do with it, as the same experiments prove. And therefore the universal adoption of iron for steam boilers, is based no doubt upon well-grounded experience, for iron is stronger and cheaper, is equally effective in raising steam, and lasts, when well made, frequently till the *fashion* has changed, and some better arrangement taken its place.

As Peclet treats of the same subject in his "Traité de la Chalure," and arrives at similar results, I have taken the liberty of introducing some of his remarks. He comes to the conclusion that in the limits of thickness generally employed, the nature and thickness of the metal are without sensible influence. "For example," he says, "in the case of steam boilers, at least for that part which does not receive the radiation from the fire, I have not made direct experiments on this subject, but the result of practice does not permit me to doubt that if the nature and thickness of the metal have any influence it is very small, for it has been long known that boilers of cast iron, of copper, and of malleable iron of the same dimensions, but in which the metals have very various thickness, give sensibly the same products in the same circumstances. This," he adds, "is a fact upon which all engineers are agreed. We might besides

easily take account of it. When the thickness of it increases or its conductibilities decrease, the temperature of the exterior surface increases. This is a fact well known, for in cast iron boilers the exterior surface often becomes red hot; and as to malleable iron boilers, the alteration which they experience from the action of the heat increases with their thickness, but as the quantity of heat which they transmit increases with the temperature of the exterior surface, we conceive the influence of the nature and thickness of the metal to be very feeble."

February 16, 1853.—DR. GEORGE WALKER ARNOTT *in the Chair.*

MR. MALCOLM M'NIEL WALKER was elected a member.

Mr. Donaldson and Mr. Taylor were admitted as members.

The following motion was proposed by Mr. W. J. Macquorn Rankine, and seconded by Dr. Walter Blackie:—

"That the office-bearers of the Society be requested to prepare and sign a memorial to the Lords of Her Majesty's Treasury, in support of the application of the Town Council of Glasgow, recommending that the Ordnance Survey of the Municipality of Glasgow be made on a scale of ten feet to one mile, and be conducted with vigour by means of an adequate supply of funds."

The motion was supported by Mr. D. M'Kain, Mr. Andrew Liddell, and Mr. William Brown, and unanimously agreed to.

The following paper was read:—

XXXIX.—*On the Natro-Boro-Calcite, or 'Tiza' of Iquique.*

By THOMAS ANDERSON, M.D.

THE object of the present communication is to bring under the notice of the Society a mineral of considerable scientific and practical interest, the former dependent on the remarkable conditions under which it is found, the latter on the possibility of its becoming an important commercial article, as a source from which boracic acid and borax may be obtained. The specimen which I have examined and analyzed is part of a considerable quantity which has been imported into this country with the view of ascertaining its mercantile value.

The mineral was originally discovered some years since, and was described and analyzed under the name of Hydro-Boro-Calcite, by an American chemist, Hayes, who found it to consist of boracic acid, lime, and water. Since then it has been analyzed by Ulex, who detected in it a quantity of soda which Hayes had overlooked, and altered its name to Natro-Boro-Calcite. With the exception of these observers no person has examined it, and it appears to have excited comparatively little attention, for the new edition of "Phillips's Mineralogy," by Brooke and Miller, published last year, contains only a very cursory notice of it.

The Natro-Boro-Calcite is found in the nitrate of soda beds of the province of Tarapaca in Peru, and is known to the natives by the name of Tiza. It occurs in rounded masses, varying from the size of a hazel-nut to that of an egg. Externally these fragments have a dull and dirty appearance, but when broken across they are found to be formed of a series of interlaced needles of a brilliant white colour and silky lustre. These crystals were extremely minute in all the pieces I have examined, but the specimen analyzed by Hayes was composed of prisms a quarter of an inch in length. According to Ulex, crystals of brogniartine are invariably present in these masses, but in all those I examined only one small crystal of that mineral was found.

The qualitative analysis indicated the presence of boracic and sulphuric acids, lime, soda, water, siliceous sand, and traces of chlorine. Ulex found also traces of nitric acid, but that I examined contained none. I have also been informed that iodine has been observed in it to the extent of 1·5 per cent., but a careful examination failed to detect it in this specimen. The method of analysis adopted was similar to that of Ulex. The water was determined by ignition, and the resulting mass dissolved in hydrochloric acid; the siliceous matters separated by filtration, and the sulphuric acid determined in the fluid. Another portion was treated with hydrofluoric acid, so as to expel the whole of the boracic acid in the state of fluoride of boron. The residual mass was then treated with sulphuric acid to convert the fluorides into sulphates, and the excess of sulphuric acid expelled by heat, the residue being moistened with ammonia and again ignited, so as to ensure absolute neutrality. The mixed sulphates of lime and soda were weighed, and the quantities of lime and sulphuric acid being determined in the ordinary way, the difference gave the amount of soda. The results were—

Water,	25·46
Boracic acid,	47·25
Lime,	15·98
Soda,	0·45
Sulphuric acid,	9·88
Chlorine,	trace.
Sand,	0·98
	100·00

This analysis gives results which accord very closely with those of Ulex, excepting that previous to his analysis he boiled the mineral with water, so as to extract the nitrate and sulphate of soda which he had detected in it, and which are obviously a mechanical mixture, and not essential to the constitution of the mineral. This was not done in my case, as the analysis was made for commercial purposes, and I was

desirous of ascertaining its exact composition as it occurs. Ulex obtained—

Water,	26·0
Lime,	15·7
Soda,	8·8
Boracic acid,	49·5
	100·0

If the small per centage of sand and sulphuric acid, and the quantity of lime or soda necessary to combine with the latter, be subtracted from my analysis, the results will be found to approximate very closely to those of Ulex, and to agree well with the formula—

$$NaO\,2BO_3 + 2CaO\,3BO_3 + 10HO.$$

The conditions under which this substance is found in loose masses in the nitrate of soda beds, give it a peculiar interest in a scientific point of view, and render it highly desirable that we should have full details regarding the whole circumstances of its occurrence. The district of Tarapaca has been as yet but little explored by scientific observers, but it would appear that it is chiefly volcanic, and it is remarkable that up to the present moment boracic acid has never been found abundantly except in volcanic districts. The commercial interest is equally great, for should the mineral prove abundant it will form an important source of borax, of which the supplies at the present time are by no means great. Nothing can be simpler than to obtain borax from the Natro-Boro-Calcite; it suffices to boil the mineral in powder with the proper proportion of carbonate of soda, filter and evaporate, when the borax is obtained in crystals and in a state of great purity. One hundred parts of the substance treated in this way yield about 130 of borax, and as the process could be performed at a cost little, if at all greater, than that incurred in the purification of the tincal or crude borax of Thibet, it is easy to see that it may become an article of considerable commercial value. Its importance will, however, greatly depend on the quantity in which it can be obtained, but on this point we have at present but little definite information. I learn from the importers that the expense of collection is considerable, and the quantity by no means large; but it would appear that no very systematic attempts have been made to obtain it in quantity, so that it may possibly be more abundant than it at present seems, and a more extended examination might lead to the discovery of larger deposits. It is possible even that it may occur not merely in masses, but disseminated in minute crystals through the soil, and if this were the case borax might be obtained in larger quantity by lixiviating with carbonate of soda the residue of the extraction of nitrate of soda. It is also worthy of inquiry whether other compounds of boracic acid and even borax itself may not be found in the same district.

The importance of such an inquiry I believe to be considerable, for borax is employed to a considerable extent in various processes in the arts, and its uses would probably be much extended if its supply increased, and price diminished, so that there might be some inducement to experimenting with it, but until this is the case it is not likely that its employment will be much extended.

Mr. Cockey, the Librarian, read the following recommendation of the Council, met as the Library Committee, and which received the approval of the Society:—

"The Library Committee has agreed to the following rules:—During the Session of the Society, new books shall not be given out till they have been laid upon the table at the first meeting after they have been received into the Library; and during the summer months, new books shall lie on the table for at least a fortnight before being given out. All the Periodicals shall lie on the table until the next number of each be received, provided the period does not exceed a month. Rare and valuable books to be divided into two classes; one class to be marked, 'Not to be given out;' and the other to be given out only on special application in writing to the Library Committee."

Mr. Cockey laid on the table a copy of Dr. Strang's collected Statistical Publications, presented to the Society by the author. Thanks voted.

March 2, 1853.—*The* PRESIDENT *in the Chair.*

MR. MALCOLM M'NIEL WALKER was admitted a member.

The following were proposed as members:—Mr. David Kirkaldy, and Mr. William Rigby.

A letter addressed to Mr. Hastie was read, acknowledging, in name of Mr. Wilson, receipt of Society's memorial to the Lords Commissioners of Her Majesty's Treasury in regard to the maps of the Ordnance Survey for the West of Scotland.

Mr. Liddell moved that the Council be authorised to appoint a deputation to proceed to Hull to attend the meeting of the British Association this year, and invite that body to visit Glasgow in 1854. Dr. G. Walker Arnott seconded the motion, which was agreed to.

Thomas Anderson, Esq., M.D., Professor of Chemistry in the University of Glasgow, read a paper "On the Changes in the Properties of Chemical Elements and the General Phenomena of Allotropy."

March 16, 1853.—*The* PRESIDENT *in the Chair.*

MR. WILLIAM RIGBY and Mr. David Kirkaldy were elected members of the Society.



Mr. Henderson was proposed as a member.

Mr. Liddell moved the following resolutions:—

"*First*—A memorial to the Lords of the Treasury, praying that they give peremptory instructions to the Commissioners of Patents to transmit transcripts of Specifications for Patent Inventions to the Chancery Office, Edinburgh, in conformity with Act of Parliament, 1852. That the Lords of the Treasury would not sanction an Act proposed to be brought into Parliament by said Commissioners, for repealing so much of the Act of 1852 as provides for a copy of each Specification being sent to Edinburgh; but rather that duplicate copies be sent to Glasgow and other large cities and towns of the empire; and further, that the surplus fund arising from the charges for Patents be applied to that purpose.

"*Second*—An Address to Prince Albert, approving of the suggestions and propositions which His Royal Highness has made of additional Libraries and Museum for the reception of Transcripts of all Patent Inventions of this and other countries, from the earliest times till the present day, to be arranged under heads, with indices.

"*Third*—A Memorial to the Commissioners of Patents, praying the Honourable Board to aid Prince Albert in the highly national work suggested by him, as the surplus funds arising from charges for Patents cannot be more beneficially or legitimately applied than in doing this, and in transmitting to the large cities and towns duplicates of the proposed Library and Museum."

The resolutions were seconded by Mr. William Murray, supported by Mr. Rankine, Mr. Jaffrey, Mr. John Joseph Griffin, and Dr. Francis Thomson, and unanimously approved of.

The President announced that in compliance with the request of the Council, Dr. Anderson, Professor of Chemistry in the University of Glasgow, had consented to become editor of the printed Proceedings of the Society.

Mr. Hart exhibited a piece of limestone from Upper Canada, with an iron spear-head imbedded in it. The specimen was obtained from a lime quarry in the river Thames, forty miles above London, and consisted of a mass of fossil shells.

A letter from Mr. Hastie was read, acknowledging receipt of the Society's Memorial to the Lords of Her Majesty's Treasury, in favour of laying down the Ordnance Survey of the city of Glasgow, on the scale of ten feet to one mile, and stating that he will transmit it to the proper authorities with his cordial recommendation.

Dr. Penny gave an account of the method of preserving meats for long voyages, as practised in Messrs. Ritchie and M'Call's establishment, London, showing the scientific principles involved in the process, and illustrating the description of numerous specimens obtained from Messrs. Ritchie and M'Call.

March 30, 1853.—*The* President *in the Chair.*

Mr. David Kirkaldy was admitted a member.

Mr. William Henderson was elected a member.

Mr. Napier exhibited specimens of Rock Salt from Carrickfergus.

Mr. Bryce briefly described the geological structure of the district in which the salt occurs, it being obtained from the upper beds of the new red sandstone.

Mr. Paul Cameron read a paper "On the Variation of the Compass, and the best means of Rectifying it;" and "On the best means of Constructing Iron Ships, with a view to a more Correct Indication of the Compass."

April 13, 1853.—*The* President *in the Chair.*

Mr. William Henderson was admitted a member.

Professor Allen Thomson described the "Anatomical Relations of the Limbs of Vertebrate Animals to each other and to the Skeleton."

April 27, 1853.—Mr. Harvey *in the Chair.*

Mr. Crum read the following paper:—

XL.—*On the Acetates and other Compounds of Alumina.*

By Walter Crum, F.R.S.

The salt from which most of the products described in the present memoir were produced, is the tersulphate of alumina, now manufactured in large quantity in the north of England, under the name of concentrated alum. It is formed by the direct action of sulphuric acid upon clay, and, after a certain purification, is evaporated and sold in cakes.

The impurity which the commercial article contains in largest quantity is potash alum. To separate that substance, advantage was taken of its insolubility in a saturated solution of the tersulphate. The cake-alum was dissolved in a quantity of boiling water insufficient for its entire solution in the cold. On cooling, the excess of sulphate of alumina, mixed with almost the whole of the potash alum, deposited, and was separated by filtration through calico. The filtered solution was then evaporated and cooled, when the tersulphate was found to crystallize in granular spongy masses. It was then drained and separated as much as possible from the mother liquor by strong pressure between numerous folds of calico. The product was re-dissolved in boiling water, and again crystallized and pressed.

A salt was thus obtained, having little more than traces of the arseni-

ous acid and the potash of the original material, together with a little iron and chloride of sodium. It has the formula

$$Al_2O_3, 3SO_3 + 18HO$$

first observed in 1825 by Boussingault in the hair-like substance found among the black schistus of the Andes of Columbia, near Bogota. He afterwards found the same substance in the volcano of Pasto, (north of Quito,) where it is formed by the action of the sulphurous vapours upon the schistus. It exists there in such quantities as to have enabled the Pastusos to become the manufacturers of chemical products for the whole country. They dissolve the alum in water, evaporate it to dryness, and form it into spherical masses like camphor, which are altogether free from iron.

The same substance is formed in this neighbourhood from a schistus containing pyrites, in which case it is mixed or combined with sulphate of iron.

The quantity of cake-alum now produced annually at Newcastle and at Sowerby-bridge, according to statistics furnished me by Mr. Wilson of Hurlet, is .....1,500 tons.
And of crystallized alum (chiefly ammoniacal) in England, 13,200 "
Do. do. do. in Scotland, 4,200 "

Total in Great Britain, ...18,900 tons.

It is estimated by Mr. Wilson that about one-fourth of this quantity is consumed by calico printers, and converted more or less completely into acetate of alumina. There is no other example of a substance so important and so extensively employed in the arts, as acetate of alumina, having been so little attended to by chemists.

INSOLUBLE ACETATES OF ALUMINA.

I. *Insoluble Binacetate of Alumina, 5 Hydrate.* An acetate of alumina was produced by mixing together strong solutions of tersulphate of alumina (purified as already described) and of acetate of lead. They were poured slowly together into a vessel surrounded with cold water, and much agitated to reduce the temperature of the mixture. To the filtrate was added sulphide of hydrogen, to precipitate the lead of the sulphate of lead which remained in solution; and acetate of baryta to throw down its sulphuric acid.

The strongest solution formed in this way contained about 5 per cent. of alumina, (from about 6 lbs. acetate of lead in an imperial gallon of the mixture.)

When such a solution (or one containing even 4 per cent. of alumina) is set aside and left at rest at a temperature of 60° or 70° Fahr., it begins after four or five days (without losing much of its transparency) to deposit upon the vessel a crust, which continues for some time to increase in

thickness. When the liquid is poured off, and the crust allowed to dry, it separates readily from the vessel in hard plates like porcelain. If the solution be not left strictly at rest, it becomes turbid after some days, and the crust is produced in a more friable state. In cold weather the solution remains unaltered for a much longer time. Heated to redness, the salt becomes black from the decomposition of part of its acetic acid, and in this state it very slightly affects the colour of moistened litmus paper, showing that it retains but little alkali. Dissolved in nitric acid and tested with nitrate of silver, it showed a trace of chlorine, and it contained also a little iron. It was freed from these impurities by processes to be afterwards detailed.

To ascertain the quantity of alumina in one of these salts, it is enough to moisten it with rectified and concentrated sulphuric acid in a platinum crucible, to evaporate the excess of acid over a spirit lamp, and then to subject it to a white heat half-an-hour in a furnace. The sulphuric acid expels acetic acid, and thereby secures the absence of charcoal. It facilitates the disengagement of water; and the powder, which, without it, becomes so pulverulent by calcination as to be with difficulty prevented from escaping, is thereby rendered coherent.

Attempts were first made to determine the acetic acid by the alkalimeter, taking potash alum of known purity for a standard, and for a comparison of results. The substance being kept in water at a boiling heat during the addition of the alkali, (soda,) and by a careful comparison of the shades of the litmus paper, appeared at first to redden that test, so long as any portion of the salt remained undecomposed. But it was found, after some practice, that at least 5 per cent. of the insoluble salt remained untouched by the alkali after all trace of acid reaction had disappeared in the process, and that within a range of 3 or 4 per cent. the method was not to be depended upon. It gave valuable assistance, however, in this investigation, in the testing of solutions of alumina, with which it gave results much more accurate than with the insoluble salts.

33·24 grains of the substance, dried first in the air, then pulverized and dried further in a stove twenty hours at 100° Fahr., were moistened with 80 grains of rectified sulphuric acid in a platinum crucible, and calcined as above. There remained 8·64 grains. When this residue was treated with water and filtered, the solution showed no trace of sulphuric acid, and was alumina nearly pure = 25·99 per cent. of the original substance.

In another experiment, 32·64 grains, dried four hours in the stove, and treated as above, left 8·4 grains of alumina = 25·74 per cent. The mean is 25·86 per cent. of alumina.

Burned with oxide of copper, 2·985 grains produced 5·526 cub. inches of dry carbonic acid = 2·612 grains, and corresponding to 50·69 per cent. of acetic acid. In another experiment 1·685 grains indicated 50·58 of acetic acid. The mean is 50·63. It was assumed that the remaining 23·51 per cent. was water. The salt was therefore composed of—

Acetic acid,	50·63
Alumina,	25·86
Water,	23·51
	100·00

The formula, Al_2O_3, $2\bar{A}$ + 5HO, requires—

Acetic acid,	51·41
Alumina,	25·91
Water,	22·68
	100·00

II. *Insoluble Binacetate of Alumina, 2 Hydrate.* If heat be applied to the strong solution of acetate of alumina, described in the previous page, it speedily becomes turbid, and deposits a heavy white powder, which falls readily to the bottom of the vessel. In a couple of days, at 100° Fahr., a considerable quantity of this powder is deposited; but in two or three hours at 160°, and in much shorter time at boiling heat, the whole of the salt is thrown down, and nothing remains in the liquid but acetic acid, excepting a trace of alumina, just distinguishable by carbonate of soda.

The precipitate has a crystalline shining appearance in the moist state, and seems, under the microscope, to consist of small oval particles of uniform size. It falls into fine powder in drying, after which, when mixed with water, it remains for a long time in suspension. Boiling water does not free it from chloride of sodium, of which it contains about 0·1 per cent.

To remove that impurity, the substance is to be dissolved with the assistance of heat in two equivalents of rectified sulphuric acid, diluted with three times its weight of water. The solution is decomposed by subacetate of lead—

$$Al_2O_3, 2\bar{A} + 2SO_3 + 2PbO, \bar{A}$$
$$= Al_2O_3, 3\bar{A} + 2(PbO, SO_3)$$

and the filtrate freed from lead and sulphuric acid as before. When heat is now applied to this solution, a precipitate is obtained, in which no trace of alkali or of chlorine can be detected.

A portion of the substance obtained at a boiling heat, and dried 24 hours at 100° Fahr., yielded 29·83 per cent. of alumina. Another portion dried 48 hours gave 30·54 per cent. Burned with oxide of copper, 1·697 grains of the salt, dried at 100° Fahr., indicated 59·87 per cent. of acetic acid. In another experiment 1·605 grains indicated 58·88 per cent. The mean is 59·37. The substance is, therefore, composed of—

Acetic acid,	59·37
Alumina,	30·18
Water,	10·45
	100·00

The formula, Al_2O_3, $2\bar{A} + 2HO$, requires—

Acetic acid,	59·51
Alumina,	29·99
Water,	10·50
	100·00

Numerous specimens of this salt were formed by precipitation from solutions of various strengths, at various temperatures below boiling water, and analyzed for acetic acid and alumina. They generally agreed in the proportions of acid and base, but varied in the absolute quantity; indicating a range of from 3 to 5 equivalents in the proportions of water. The two salts which have been described as deposited, one at 60° and the other at a boiling heat, gave results nearly uniform.

When heat is applied to a solution of teracetate of alumina weaker than that which has been described; to one, for example, containing 3 per cent. of alumina, it also yields the insoluble binacetate, but, in that case, not acetic acid alone, but a considerable quantity of acetate of alumina remains in the solution. Solutions containing 2 per cent. of alumina are precipitated by boiling, if they have been kept some weeks, but not if recently prepared. It appears from these experiments, that the presence of free acetic acid favours, in some way, the production of the insoluble binacetate of alumina from a solution of teracetate. It was found, accordingly, that a solution of teracetate containing $\frac{3}{4}$ per cent. of alumina, and which could not therefore be precipitated by boiling, acquired that property when made to contain acetic acid equal to a solution of teracetate, with 4 per cent. of alumina.

In whichever way deposited, this substance is exceedingly insoluble in water, either cold or hot, and it is equally insoluble in acetic acid. When one part of it is digested for an hour and a-half in two hundred parts of boiling water, it dissolves; and the solution consists partly of soluble binacetate of alumina, and partly of acetic acid and the bihydrate of alumina to be afterwards described.

It dissolves in two equivalents of sulphuric acid, or of hydrochloric, or nitric acid, forming bisalts of alumina, and liberating the two atoms of acetic acid. It also dissolves in a strong solution of tersulphate of alumina, with the assistance of heat, forming bisulphate of alumina and free acetic acid.

$$2(Al_2O_3, 3SO_3) + Al_2O_3, 2\bar{A} = 3(Al_2O_3, 2SO_3) + 2\bar{A}$$

The mixture does not precipitate on the addition of water like the bisulphate alone, and must therefore be changed by water into tersulphate and binacetate of alumina.

$$2(Al_2O_3, 3SO_3) + Al_2O_3, 2\bar{A}$$

A solution of potash alum dissolved this binacetate in the same circumstances, but on the heat being continued for some time, a precipitate

was formed anew, which appeared from a qualitative analysis to be the monosulphate of alumina and potash,

$$KO,SO_3 + 3(Al_2O_3, SO_3)$$

the substance which is formed when hydrate of alumina is boiled with alum. The same substance is found native, under the name of alum-stone, at La Tolfa, the seat of the celebrated manufacture of Roman alum, near Civita Vecchia; and was ascertained, by Collet Descotils, to be formed of—

$$KO, SO_3 + 3(Al_2O_3, SO_3) + 9HO$$

SOLUBLE ACETATES OF ALUMINA.

Soluble Binacetate of Alumina. $Al_2O_3, 2\bar{A} + 4HO$. Notwithstanding the tendency of a concentrated solution of the ter- acetate of alumina to deposit the insoluble salt, it may be evaporated, with certain precautions, to a dry substance soluble in water. For this purpose it must be spread very thin over sheets of glass, or of porcelain, exposed to a heat not exceeding 100° Fahr., and, as it runs together into drops, like water upon an oiled surface, it must be constantly rubbed with a thin platinum or silver knife. If these precautions are neglected, a mixture is obtained of the insoluble with a soluble acetate.

The soluble salt is thus produced in scales, having the appearance of gum when moistened, and leaving no residue when dissolved in water.

For analysis it was reduced to powder, and dried in the air twenty-four hours, at the temperature of 100° Fahr. The alkalimetric method was first employed, and it indicated 54·8 per cent. of acetic acid; but the more accurate method, by oxide of copper, gave 55·82 per cent. of acid. In experiments for the alumina 26·4 per cent. was obtained. The composition of the substance was thus—

Acetic acid,	55·82
Alumina,	26·40
Water,	17·78
	100·00

But the solution from which this subtance was obtained was a teracetate, and contained for the same quantity of alumina,

Acetic acid, ... 78·58

It lost, therefore, during gentle evaporation, nearly one-third of its acetic acid, and was reduced almost to a binacetate, the formula of which—

$Al_2O_3, 2\bar{A} + 4HO$—requires—

Acetic acid,	52·38
Alumina,	26·40
Water,	18·49
	97·27

The excess of acetic acid = 3·44 per cent. over the proportions of a binacetate, was presumed to be free acid adhering to the product. To remove that excess, a portion of it was heated in a water bath, and afterwards treated with water. A considerable quantity of insoluble matter was left, and the solution on being evaporated to dryness, at a gentle heat, produced a soluble acetate, of which 100 parts contained—

of acetic acid,	49·55
and of alumina,	32·47

corresponding, therefore, in composition, to a sesquiacetate; but it can only be looked upon as an accidental mixture, for every fresh portion that was produced in a similar way gave a different result—the amount of acid depending upon the extent and duration of the heat.

The binacetate of alumina may be produced at once in solution; and as it is the most suitable combination from which to form the dry soluble binacetate, as well as other bodies, I shall describe particularly the manner of obtaining it. Dissolve 24 parts of precipitated binacetate of alumina in 15 of rectified sulphuric acid, and 40 of water. Dilute further with 80 parts of water, and add carbonate of lead (about 44 parts) to precipitate the sulphuric acid.

$$Al_2O_3, 2\bar{A} + 2SO_3 + 2(PbO, CO_2) = Al_2O_3, 2\bar{A} + 2(PbO, SO_3) + 2CO_2$$

Filter the solution, and pass sulphide of hydrogen through it until it ceases to precipitate lead, and then add acetate of baryta, so long as it is precipitated by the sulphuric acid of the sulphate of lead which had remained in solution. When in this state, if the mixture be well agitated for half an hour in an open vessel, the excess of sulphide of hydrogen will be removed, and it may be filtered without the danger of the filtrate becoming afterwards milky, from the effects of the sulphide.

A solution of binacetate of alumina is thus obtained, containing about 5 per cent. of alumina, and a minute portion of iron; the last traces of which it was, for a long time, difficult to remove from these acetates. Iron is found in all the solutions, and in both of the insoluble binacetates. Traces of it exist even in the binacetate which has been freed, by a second precipitation, from all its other impurities. It was at last observed that a solution of binacetate of alumina, at 5 per cent., which is strong enough to form, after some time, a crust of the binacetate, deposited its iron along with the first portions of the crust, and left the solution altogether free from that impurity. The teracetate of alumina

does not deposit its iron in like circumstances; a difference which may be accounted for from the fact that the binacetate of the sesquioxide of iron is more readily decomposed than the teracetate of that base.

Spread very thinly over a sheet of glass, it evaporates at 60° or at 100° Fahr., without running into large drops so much as the teracetate, and without having an equal tendency to produce the insoluble binacetate. The scales into which it forms in drying are transparent and soluble in water.

Burned with oxide of copper for acetic acid, and calcined for alumina, 100 parts were found to contain—

Acetic acid,.....55·21
Alumina, ...31·31

But the alkalimeter had shown, that in the solution the same quantity of alumina had been combined with—

Acetic acid,.....59·13

More than 4 per cent. therefore had been lost by evaporation at 100° Fahr.

After some other attempts, I was obliged to conclude that the only way to obtain the dry soluble binacetate in something like atomic proportions, was the unsatisfactory one of evaporating in the air a solution of binacetate, mixed with such a proportion of acetic acid or of teracetate as is found by trial to produce it.

On the question, as to which of the soluble acetates of alumina can be considered as a definite compound, it has already been stated of the teracetate, that when evaporated rapidly enough, and at a heat just low enough to prevent the formation of the insoluble salt, it leaves a product whose composition is nearly that of a binacetate. It may be added, that the solution of teracetate gives off acetic acid as freely in the cold, as if a third part of its acid were free. On making an experiment with two solutions of acetate of lead—one of which was decomposed by sulphuric acid, and the other (which was three times as strong) by tersulphate of alumina—it was found that the aluminous solution gave a smell of acetic acid considerably stronger than that in which the acid was known to be free. It may be doubted, then, whether there exists a combination of acetic acid with alumina corresponding to the tersulphate of alumina. The solution of binacetate has no smell of acetic acid at ordinary temperatures.

BIHYDRATE OF ALUMINA SOLUBLE IN WATER.

$$Al_2O_3 + 2HO.$$

Hydrate from the soluble Binacetate.—By the continued action of heat on a weak solution of binacetate of alumina, a permanent separation of the constituents of the salt takes place, although no acid escapes, and no

alumina is precipitated. The properties of the alumina are at the same time materially changed.

A solution of binacetate of alumina, diluted so as to contain not more than 1 part of alumina in 200 of water, was placed in a close vessel which was immersed to the neck in boiling water, and kept in that state day and night for ten days. It had then nearly lost the astringent taste of alum, and acquired the taste of acetic acid. The liquid was now placed in a wide and shallow vessel, where it was kept at a uniform depth of a quarter of an inch; and on heat being applied to make it boil briskly over the whole surface, the acetic acid was driven off in about an hour and a-half, so as to be no longer sensible to litmus paper. The liquid in this operation should not contain more than 1 part of alumina in 400 of water, and the loss from evaporation is supplied by continual additions of water.

The solution thus obtained is nearly as transparent and limpid as before the loss of its acid. By longer boiling, and particularly by concentration, it becomes more and more gummy; a quality of which it is partially deprived by acetic acid. It is altogether tasteless.

When one grain of sulphuric acid (SO_3) in 1000 grains of water, is mixed with 8000 grains of the solution, containing 20 grains of alumina, the whole is converted into a solid transparent jelly. By pressure in a bag the liquid part of this jelly is readily separated from the solid; whose volume in the compressed state, is not more than $\frac{1}{60}$ or $\frac{1}{70}$ of the volume of the jelly. On examination it was found that the solid part of the coagulum contained almost the whole of the sulphuric acid which had produced it—about one equivalent of sulphuric acid to fifteen of alumina.

One atom of citric acid (tribasic) coagulates as powerfully as three atoms of sulphuric acid; and tartaric acid (bibasic) as much as two. Two atoms of oxalic acid are required to produce the same effect as one of sulphuric acid. Of muriatic and nitric acids not less than 300 equivalents must be employed to produce an effect equal to that of one equivalent of sulphuric acid.

Of the other acids which have been tried, the chromic, molybdic, racemic, suberic, salicylic, benzoic, gallic, lactic, cinnamic, butyric, valerianic, carbazotic, camphoric, uric, meconic, comenic and hemipinic acids all coagulate the solution; but their exact power has not been ascertained.

The acetic, formic, boracic, arsenious, and cyanuric acids do not coagulate, at least when moderately concentrated; and, of the opium acids, for a supply of which I am indebted to Professor Anderson, the pyromeconic and opianic acids also do not coagulate.

One grain of potash in 1000 grains of water coagulates 9000 grains of the solution—a proportion which gives about 1 equivalent of potash to 20 of alumina. The mixture has a slight alkaline reaction. Soda, ammonia, and lime have an equally powerful effect. The coagulum they produce is partially re-dissolved when the alkali is saturated by acetic or by muriatic acid. The salts which are thus formed render the solution *somewhat* oily.

A boiling solution of potash or soda dissolves the coagalum, and at the same time changes it into the ordinary alumina, which is thrown down in the state of terhydrate when the alkali is saturated by an acid.

Oil of vitriol also dissolves the solid part of the coagulum, and the same substance dried; especially when assisted by heat. Strong muriatic acid, at a boiling heat, does the same, though with greater difficulty; and the products are the ordinary sulphate and muriate of alumina.

Large quantities of the acetic salts may be added before they coagulate the aluminous solution.* When the solid part of the coagulum produced by a strong solution of acetate of soda was afterwards freed from that salt by pressure, it re-dissolved in pure water, and the solution was again coagulated by a fresh addition of the salt. An experiment with acetate of lime gave the same result.

The nitrates and chlorides coagulate also with great difficulty.

Solutions of sulphate of soda, magnesia, and lime coagulate as readily as a liquid containing the same quantity of sulphuric acid in the free state. On examining one of these mixtures, the sulphuric acid was found in the solid part of the coagulum, as before; and the mixture remained neutral.

A small spoonful taken into the mouth becomes immediately solid from the effect of the saliva.

The digested solution of alumina which has not been deprived of its acetic acid by boiling, requires about twice as much sulphuric acid to coagulate it as does the boiled solution.

One of the most characteristic properties of the digested and altered acetate of alumina is its loss of the power of acting as a mordant. The ordinary acetate, as is well known, forms a yellow opaque precipitate with decoction of quercitron. That which has been thoroughly digested is merely coagulated by that decoction,—the colour of which is but little altered, and the coagulum is translucent. The same effect is produced with decoctions of logwood, brazil-wood, &c.

A quantity of the solution of hydrate of alumina was evaporated to dryness at the heat of boiling water. After being pulverised and again submitted to the same heat, it was moistened with sulphuric acid as before described, and heated to whiteness. It lost 25·67 per cent. of its weight.

Hydrate from the Insoluble Binacetate.—It has been already mentioned that when the precipitated binacetate is kept for an hour or two in 200 parts of boiling water, it is changed into the soluble binacetate. It must be constantly agitated during that time. The substance so produced may be converted, like the original solution into the peculiar bihydrate. Thirty to thirty-six hours digestion is sufficient to complete

* Hence, in preparing the binacetate of alumina which is to be used in obtaining the bihydrate, it is better to employ an excess of acetate of baryta, than to leave in the solution the slightest trace of sulphuric acid.

the change, for after that time the solution has no longer any taste of alum, and the power of coagulating with acids does not increase.

6·93 grs. of this hydrate, which had been dried at a steam heat, were moistened with sulphuric acid, which was then gradually expelled by a spirit-lamp, and the residue, when kept forty minutes at a white heat, left 5·20 grs. of alumina, indicating 24·97 per cent. of water. The mean of this and the previous experiment gives for the composition of the hydrate—

Alumina,..	74·68
Water,..	25·32
	100·00

A bihydrate requires—

Alumina,..	74·06
Water,..	25·94
	100·00

Acetates of Sesquioxide of Iron.

The analogy between the salts of alumina and those of the red oxide of iron, induced me to inquire whether there might not exist allotropic acetates of iron, corresponding with those of alumina.

A solution of sulphate of iron, to which was added half an equivalent of sulphuric acid, was acted upon by strong nitric acid, and the tersulphate so obtained was converted into the teracetate by decomposition with neutral acetate of lead. A binacetate was also produced by decomposing with a mixture of acetate and carbonate of lead. Both solutions had the same intense red colour which is peculiar to the peracetate of iron.

The teracetate of iron, whether concentrated or weak, is little liable to decomposition in the cold. Boiling causes the deposition of a hydrated peroxide, but the decomposition is only partial, and the hydrate difficult to collect.

The binacetate soon begins to let fall an oxide in the cold; and at the heat of boiling water a complete separation takes place. A rich deep-coloured hydrate goes readily down, and the whole of the acetic acid remains in the liquid, which is perfectly colourless. No allotropic acetates of iron, corresponding to those of alumina, appear therefore to exist.

Bibasic Sulphate of Alumina.

$$2Al_2O_3, SO_3 + 10HO.$$

It is well known that when teracetate of alumina is boiled along with sulphate of potash, a gelatinous precipitate is formed, which redissolves when the solution becomes cold. M. Kœchlin-Schouch* found it to be a subsulphate of alumina.

* Sur le Mordant Rouge, &c. Bulletin de la Societé Industrielle de Mulhausen I. 299.

When alum, dissolved in six times its weight of water, was treated with three equivalents of acetate of lead—enough to decompose its tersulphate of alumina, but not the sulphate of potash, a large proportion (74 per cent.) of the sulphate of potash was found to be carried down by the sulphate of lead formed in the process. To a solution so prepared, the loss was restored by an addition of sulphate of potash, and a mixture formed of—

$$KO, SO_3 \text{ together with } Al_2O_3, 3\bar{A}$$

This solution, diluted so as to contain about 0·3 per cent. of alumina, (from 5 oz. of alum per gallon,) was exposed to heat. At about 90° Fahr., the gelatinous precipitate began to form; and after two hours boiling, when scarcely a trace of alumina remained in solution, it was thrown upon a filter of calico, which was kept hot within a steam pan until the filtrate passed through. The collected precipitate was pressed between numerous folds of bleached calico, and brought gradually to the state of a stiff clay occupying not more than $\frac{1}{40}$ of the volume of the original solution. It was then divided into portions which could conveniently be shaken and thoroughly mixed in bottles with quantities of water equal in all to the original solution. The mixture was filtered as before, at nearly a boiling heat, and pressed between folds of calico; and another repetition having been made of the same process, the precipitate was deprived of every admixture of soluble matter.

A portion being dissolved in nitric acid and filtered, gave an abundant precipitate with nitrate of baryta. Exposed to a white heat, and the residue pulverized and boiled in water, the filtered liquor gave no longer a precipitate with nitrate of baryta, and no indication, or a very doubtful one, of alkali to litmus paper; proving that the sulphuric acid indicated by the first test had been combined, not with potash, but with alumina. Mixed with sulphuric acid the substance gave no smell of acetic acid, and when burned with oxide of copper it yielded no more carbonic acid than could readily be accounted for by the few fibres of cotton wool that were to be distinguished in the liquid. The substance dried into a hard, whitish, semi-transparent matter, easily pulverized.

For analysis 33·93 grains dried at 100° Fahr. were dissolved in muriatic acid, and the solution was exactly decomposed at a boiling heat by 15·25 grains of chloride of barium.* This indicates 5·867 grains, or 17·29 per

* The heat in this case greatly facilitates the arrival at the nearest point of decomposition, particularly by enabling the filter to furnish, immediately, clear portions, from time to time, for testing.

Where nitrate of baryta is employed for decomposing a sulphate, it was observed by Mitscherlich that a quantity of the nitrate was taken down unaltered by the sulphate of baryta. This source of error, which amounts to not less than 5 per cent. of the quantity of sulphuric acid when the experiment is performed in the cold, is entirely obviated at a boiling heat.

Boiling, however, does not render the decomposition of a sulphuric salt by a

cent. of sulphuric acid. 19·715 grains mixed with 16 grains sulphuric acid in a platinum crucible were first heated with a spirit lamp, and then exposed to a white heat. It left 8·51 grains = 43·16 per cent. of alumina. Another experiment gave 42·94 per cent. The mean is 43·05. The composition of the substance is therefore—

Sulphuric acid,	17·29	SO_3	17·18
Alumina,	43·05	$2Al_2O_3$	44·16
Water,	39·66	10HO	38·66
	100·00		100·00

When the solution from alum and sugar of lead was heated before the restoration of its sulphate of potash, a very slight precipitate was formed. The addition of that salt completed the precipitation, but the substance thrown down was more transparent, and dried into a brownish, horny-looking matter. Its analysis, however, gave nearly the same results.

Sulphuric acid,	17·23
Alumina,	43·51
Water,	39·26
	100·00

But when the solution was boiled for two hours before the addition of the sulphate of potash, a quantity of alumina seems to have gone down with the sub-sulphate, for its analysis gave in 100 parts

Sulphuric acid,	13·73
Alumina,	50·71

It had an appearance similar to the preceding product. In the moist state the sub-sulphate is soluble in cold acetic acid, as well as in the mineral acids. It dissolves in three atoms of sulphuric acid to form two of the bisulphate, which, on the addition of water is resolved into the tersulphate of alumina, and the insoluble monosulphate.

$$2Al_2O_3,SO_3+3SO_3 = 2(Al_2O_3,\ 2SO_3.)$$
$$= Al_2O_3,\ SO_3+Al_2O_3,\ 3SO_3.$$

The same mixture of teracetate of alumina and sulphate of potash, which in a diluted state produces subsulphate of alumina on the application of heat, gives a precipitate of binacetate of alumina when heat is applied to it in a concentrated state. A quantity of sulphate of potash, however, adheres to the insoluble binacetate even after abundant washing.

salt of baryta complete without a considerable excess of the barytic salt, and *vice versa*. The point sought for in the present instance is that at which *an equal precipitate is formed on adding an excess of either salt.*

PRECIPITATE FROM ACETATE OF ALUMINA WITH CHLORIDE OF SODIUM.

A solution of teracetate of alumina was produced from 1 pound acetate of lead in 10 pounds water, decomposed by tersulphate of alumina. After purification, common salt was added to it in the proportion of one equivalent to one of tersulphate of alumina. The solution, when heated in the water bath, became of an opaque white from the deposition of a powder so very fine that the mixture passed through the filter almost without change. Neither heat, nor any other application could alter this character. It was left at rest, and after some weeks, the liquid having become nearly clear, was poured off, and the precipitate, which had contracted into 1-64th of the bulk of the liquid, was mixed again with fresh water 11 times its volume. After several weeks more, the precipitate fell again to the bottom of the vessel, when it was mixed with a third portion of fresh water, which again, after four weeks, was decanted, and the precipitate dried in a capsule at 100° Fahr.

On analysis it yielded the following results—I do not give the details of the experiments, not having made out a formula which can be stated in atomic proportions:—

Alumina,	44·66
Acetic acid,	21·96
Hydrochloric acid,	5·51
Water,	25·90
	98·03
Chloride of sodium,	1·97
	100·00

Nitrate of potash forms a precipitate when heated with acetate of alumina, similar in appearance to that from common salt, but it was not particularly examined.

CONCLUSIONS.

1. The aluminous solution obtained by decomposing pure tersulphate of alumina with the neutral acetate of lead consists, I believe, of a mixture of binacetate of alumina, with an equivalent of free acetic acid. No true teracetate of alumina appears to exist.

2. When means are taken to evaporate the preceding aluminous solution at a low temperature with sufficient rapidity, a dry substance is obtained, which may be redissolved easily and entirely by water. This is the binacetate of alumina, ($Al_2 O_3$, 2 $C_4 H_3 O_3$ + 4 HO), in which the alumina retains all its usual properties.

3. When the first aluminous solution, containing not less than 4 or 5 per cent. of alumina, is left for some days in the cold, a salt is deposited in the form of a white crust, which is an allotropic binacetate of alumina, insoluble in water. Heat effects the same change in the

aluminous solution more rapidly, and the new acetate then precipitates as a granular powder. At the boiling temperature the liquid is thus deprived, in about half an hour, of the whole of its alumina, which goes down with two-thirds of the acetic acid; leaving one-third in the liquid.

4. The red acetates of iron treated in a similar manner do not produce corresponding isomeric binacetates. By heating the binacetate of the sesquioxide there is a total separation of acid and base. The facility with which the binacetate is decomposed, even in the cold, furnished the means of freeing the solution of binacetate of alumina of traces of iron which could not otherwise be separated from it.

5. The soluble binacetate of alumina is decomposed by heat, and affords a new and remarkable product. When a dilute solution of that salt is exposed to heat for several days, the whole acetic acid appears to become free, and the alumina to pass into an allotropic condition, in which, although it remains in solution, it ceases to be capable of acting as a mordant, or entering into any other definite combination. When the acetic acid is expelled by boiling, the alumina, in its altered state, remains alone dissolved in pure water. It is more soluble, however, in acetic acid. The allotropic hydrate of alumina retains two equivalents of water when dried at the heat of boiling water. Its solution is coagulated, more or less powerfully by the mineral, and most of the vegetable acids and their salts; by the alkalies, and by decoctions of dyewoods. The coagulum which is formed by the various acids, is not re-dissolved when they are added in excess. The solid part of the coagulum yields, however, to the continued action of oil of vitriol, especially if assisted by heat, and the result is the ordinary sulphate of alumina. Boiling potash also dissolves it, and changes it into the ordinary terhydrate. Its coagulum with dyewoods has the colour of the infusion, but is translucent and entirely different from the dense opaque lakes which ordinary alumina forms with the same colouring matters.

6. The insoluble binacetate of alumina, when digested in a large quantity of water, is gradually changed into the soluble binacetate; of which a part, however, becomes decomposed during the process into acetic acid, and the allotropic bihydrate of alumina.

7. The precipitate which is formed on the application of heat to a mixed solution of acetate of alumina and sulphate of potash, and which is soluble in cold acetic acid, is a bibasic sulphate of alumina.

The Society, on the recommendation of the Council, agreed to meet next Wednesday night for the purpose of overtaking arrears of business.

May 4, 1853.—*The* PRESIDENT *in the Chair.*

THE concluding meeting of the session was held this evening.

Mr. Thomas Dawson and Mr. Matthew Bell were requested to audit the Treasurer's Accounts before the opening of next session.

The Librarian laid on the table a copy of Memoirs of the Literary and Philosophical Society of Manchester, presented by the Society. Thanks voted.

Mr. W. J. Macquorn Rankine described a contrivance named "A Railway-Train-Signal-Rope-Slack Gatherer."

Mr. James R. Napier read a paper "On the Saving of Fuel in Furnaces."

The following paper was read:—

XLI.—*Remarks upon Sandstones used for Building, &c.*

By Mr. J. NAPIER, Chemist, Partick.

THERE are few but will have observed, while walking along our newly-formed streets, that many of the buildings that have been erected within these ten or fifteen years, have stones in them, especially on the ashlar fronts, that are rapidly corroding and destroying materially the beauty and appearance of the structure. There will also be observed that this decay or corrosion is very general under projections, or in such circumstances as keep them entirely in the shade.

A few months ago, having occasion to superintend some erections in the neighbourhood, curiosity led me to inquire of the practical men whether it were not possible to detect stones that were liable to corrode previous to being put into a prominent part of a building, and so save the heart-burnings a proprietor must feel on finding that here and there upon the front wall, and many times on expensively-wrought work, are certain stones or parts of a stone that begin to moulder ere his house is tenanted. But my inquiries led to the conclusion that nothing could be done nor any good reason given as to the cause of the decay, although not a few assured me they could tell by the mere *feel* whether a stone would last or not; but this means of testing, I was told, could not be communicated to any person not practically acquainted with the trade, and was seldom put into practice in erecting houses. Such were the circumstances that suggested the idea of endeavouring to find if a chemical examination would assist in determining the cause, and lead to any remedy. In this inquiry I have experienced much the want of data for comparison. Although making analyses of stones from all the various quarries about, I could not find specimens of tried stones, of stones that had been in buildings and exposed, and the quality either good or bad determined: hence my investigations are far from satisfactory, but I am thus bringing it forward in the present unfinished state as a call upon builders or architects to send me specimens of known qualities, so that the experiments may be more thoroughly carried out, and see if it is within the range of chemical or mechanical testing.

Sandstones are of considerable variety both in the size of the grains

which compose them and the circumstances in which they have been deposited. Sometimes they have a laminated structure, and between these laminæ are deposits of mica in very fine scales. Stones of this class are often placed into a wall with their laminæ in a vertical position. When this is done such stones are found to scale in flakes, and should therefore always be laid upon bed; but even in stones that have no apparent laminæ, but seem one solid mass, still they have a stratified structure, and when not built into the wall in what is termed their natural bed—having the strata in a horizontal position—they are always more liable to decay.

Freestone being simply sand particles cemented together under pressure, and corrosion or decay being the dissolving or loosening of this cement, my first inquiry was to find out what the cementing material was. I had formed an opinion that this in all probability was either lime or iron, and that the carbonate of lime being soluble in water impregnated with carbonic acid, the lime would be liable to be dissolved out by rain water, which generally contains that acid, or the acid might be absorbed from the air by water in the stone. These ideas were strengthened a little by finding that some of the stones contained iron as a carbonate, which by exposure was converted into a peroxide, and which might thus yield its carbonic acid to the lime, and by converting it into a bicarbonate render it soluble in water; but subsequent inquiry brought out so many circumstances to be looked to as made these suppositions not so prominent.

It often happens that stones most exposed stand best, or what is the same thing, the same stone will decay most where least exposed, as for instance pillars are often found to corrode most rapidly on the inside, or least exposed parts. Sometimes it is observed that a stone wasting away is very red in colour, and a conclusion come to that it is the iron, which being oxidised loosens the particles, an idea very tenable, but then probably in the same building are stones equally red, and containing more iron than the other, which has also peroxidised, and no decay taking place; and there may be also a stone a few feet off of marked whiteness, mouldering so rapidly that if the hand be drawn over it, a shower of sand particles fall, while the hand is stained as if by clay. These facts render the inquiry interesting, and increases the difficulty.

Sandstone, as I have already intimated, is not a chemical compound, such as slate or felspar, but a mechanical mixture, composed of the debris of chemical compounds mixed and cemented together. And to take the mass and analyse them as one compound, would be to give a false data to reason from. I therefore endeavoured first to separate this mixture mechanically.

In looking at a piece of sandstone through a lens, every particle of silica seems coated over with some white, opaque matter, resembling clay or lime. If this is connected with the binding of these sand particles, then, what under ordinary circumstances will dissolve this away? I

suspended several pieces of stone on the surface of distilled water for several days, agitating the water occasionally, and at the termination of the experiment found a good quantity of sand particles had loosened from the stone and fallen off, and the water afterwards contained a trace of lime and magnesia, and was white by a clay being suspended in it. Repeating this experiment with water containing carbonic acid, the disintegration of the stone was more, and so was the lime in the water, but the magnesia was not perceptible: there was also clay present. I then took a piece of dry stone, ground it fine in a mortar, and placed the powder in water, the sand particles sunk rapidly to the bottom, while the clay remained suspended, and by several washings and decantings the sand and clay could be approximately separated; but there being also mixed up in the stone, a great quantity of mica in fine scales, part of which floated with the clay, a perfect separation of sand and clay could not be made in this way. I then took the finely ground stone and placed it upon a stout piece of flannel cloth as a filter, and poured water over it until the water passing through was clear; by this means the clay was easily separated, and the remaining powder when dried had no longer the dusty appearance referred to, the quartz being transparent and mixed with scales of mica; and the dusty portion washed off and passed through the filter is mostly all China clay, finding in this way that the clay is often upwards of twenty per cent. of the weight of the stone; I have no doubt it may form an important part in the tear and wear of the stone. I have as yet found no easy means of separating mechanically the mica from the sand grains, and therefore cannot give their relative proportion, but they are variable.

Knowing that practical men consider that the best stones are generally found at the lowest part of the stratum or seam, I conceived that stones found at different depths might contain different quantities of clay, and so lead to some data, but the few trials I had on this head did not give any regularity in the variation, as in some quarries the greatest quantity of clay was found in the stone at the bottom of the stratum, and in others at the top part of the seam, as, for instance, in one quarry a piece 2 feet down gave 20 per cent. of clay, and a piece 8 feet down gave only 14·6 per cent. of clay; while in another quarry not above half a mile distant from that referred to, a piece 2 feet down gave 8·5 per cent. clay, and a piece 8 feet down gave 20 per cent. clay. However, my experiments upon this part of the inquiry have not extended beyond these two trials. Another question suggested itself.

In washing out the clay, were the lime, iron, and other binding ingredients in the stone also washed out? In answer to this I found that in general a great quantity of the lime and iron were washed away, but in no case was the whole of either lime or iron washed out with the clay; the following analyses will give the general character of these washings, taking the average composition of the stone before washing:—

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Silica and mica,	77·4
Clay,	15·6
Lime,	3·8
Protoxide iron,	2·2
Magnesia,	1·
	100·0

After washing these are—

SAND WASHED.		CLAY SEPARATED.	
Silica and Mica,	94·6	Clay,	88·6
Protoxide iron,	1·2	Protox. iron,	5·3
Lime,	3·8	Lime,	4·0
Magnesia,	·4	Magnesia,	2·1
	100·0		100·0

All this lime is not from the mica, and shows that lime adheres to the quartz with great force, and gives cause for suspecting that the clay may have a tendency to prevent the proper binding of the quartz particles; and this idea is somewhat borne out by the statement made by the Commissioners appointed to inquire into building stones for the New Houses of Parliament. They say in reference to Edinburgh:—

"Modern buildings erected from the Craigleith quarry, none of them exhibit any appearance of decomposition, with the exception of ferruginous stains which are produced upon some stones. Amongst the oldest is the Registry Office, which is of Craigleith, and built above 60 years since: it is in a perfect state."

Now in turning to the analysis given of this stone by the late Professor Daniell at the time the Commissioners made their inquiry, it stands as under:—

Silica,	98·3
Lime,	1·1
Iron and alumina,	·6
	100·0

Here we have no clay mentioned, and although the quantity of lime is very small, still as nothing intervened it may have formed a perfect cement along with the iron present. And we may here mention, while treating of this particular stone, that the Craigleith quarry seems to produce a different quality now than it did 14 years ago, when the above analysis was made, that stone, according to the specimen I have from Craigleith, has almost none of the characters of a sandstone; it is more compact than granular, and has a vitreous crystalline appearance, and possesses every character of a lasting stone. Its composition is nearly as under:—

Silica,	65·4
Clay,	8·5
Lime,	19·8
Protoxide iron,	1·2
Magnesia,	trace.
Water,	3·2
	98·1

From the fineness of the grain of this stone, the washing out the clay is not so perfect, but washed as carefully as I could, to see if lime was washed off, the results were—

The washed portion left on filter.		Clay portion passing through filter.	
Insoluble,	88·8	Insoluble,	50·
Oxide iron,	·8	Oxide iron,	1·6
Lime,	10·0	Lime,	48·
	99·6		99·6

I will now give the chemical character of the sandstone from the principal quarries in the neighbourhood of Glasgow, premising, that although I have made several analyses of stones from every quarry, their sameness renders it unnecessary to give every result, and will therefore only give the average of each kind or quality, with the uses for which each stone is more especially applied, and there being few remarks required on individual analyses, I will subjoin them in a tabular form. I must remark that in these analyses the clay was separated in the manner stated above, the only chemical testing was for lime, iron, magnesia, and sulphur. Many stones contain seams and thread-like fibres of carbonaceous matters, which were avoided in the testing in the following table:—

Locality.	Used for	Mica and Silica.	Clay.	Carb. Lime.	Oxide Iron.	Carb. Magn.	Natural Water.	Absorbing Water, per cub. ft	Remarks.
Garscube,	Founds,	77·8	8·2	6·4	2·8	1·6	3·2	½ gal.	Hard cl. gra.
"	Ashlar,	81·2	13·,	2·1	·8	·3	2·5	1·2 —	Fine grain.
"	Ruble,	80·3	11·4	2·4	2·8	·8	2·3	1·3 —	Black veins through it.
Netherton,	Founds,	77·	15·	2·4	2·	2·8	4·	0·7 —	
"	Ashlar,	78·5	15·'	2·8	1·6	2·	5·	0·8 —	
"	Ruble,	85·	12·6	1·6	·8	—	4·	1· —	Black veins.
Kenmuir,	Ashlar,	83·3	11·8	2·3	1·6	1·	4·	1·5 —	Fine grained.
Gifnock,	Ashlar,	79·8	12·9	4·	2·8	·5	3·	1·2 —	Fine grained.
Hillhead,		75·6	20·	2·	1·2	·8	6·4	1· —	Top of seam coarse gra.
"		79·5	14·6	3·2	1·6	·8	4·	1· —	Bot. of seam.
New Bridge,	Ashlar,	83·	10·	3·6	1·3	1·0	5·5	1·4 —	
Partick,		88·5	8·5	1·6	·8	·2	7·	1·3 —	Top of seam
"		73·4	20·	3·2	2·	1·2	6·	1·5 —	Bot. of seam coarse gra.

In the above table no notice is taken of sulphur. I have often found traces of sulphur in the ordinary stone, and in particular pieces of stone a considerable quantity, but I have not in general found it in such proportion as to make me suspect it having anything to do with the destruction of the stone, being a mere trace in 100 grains. Sulphuret of iron in the stone might cause decay by efflorescence, but I have generally found the iron to exist as a carbonate.

The application of these analyses to useful purposes, or to the wearing quality of a stone, is as yet unknown to me, owing to the want of data. I was, however, kindly supplied by a builder with four specimens, from a house he was taking down. Two of these were from stones that had corroded very much, the other two had stood the wear well, all having been placed under the same circumstances for upwards of 20 years.

The first sample tried has not corroded all over, but in lines, forming deep ruts or furrows in the stone. This gave by analysis—

Silica and mica,	73·6
Clay,	22·0
Peroxide iron,	2·8
Lime,	1·2
Magnesia,	trace.
	99·6

No. 2 had corroded all over, mouldering into sand. It gave—

Silica and mica,	77·0
Clay,	20·0
Peroxide iron,	1·4
Lime,	1·6
Magnesia,	trace.
	100·0

No. 3 stood well, and was a slight reddish colour. It gave—

Silica and mica,	90·2
Clay,	6·8
Lime,	2·2
Peroxide iron,	·8
	100·0

No. 4 stood well, was white, gave—

Silica and mica,	89.2
Clay,	8·3
Lime,	1·8
Peroxide iron,	·4
	99·7

Were we warranted in drawing any conclusion from these few analyses there is but one to make, namely, that the presence of a great quantity of clay with little lime prevents the proper adhesion of the sand particles.*

As regards the probable cause of stones decaying more rapidly in the shade than exposed to a free current of air and sunshine, the general opinion is that it is caused by want of evaporation. A good stone in a few days' exposure takes upon it a sort of skin, the iron on the surface peroxidises, and the face of the stone is clean and apparently washed-like; but it is otherwise with a bad stone, if the hand be drawn over it in dry weather it seems, and really is, dusty, the particles falling off by the friction of the hand, and this often in exposed parts where evaporation is the same as with other good stones. Nevertheless, the constant state of moisture that stones are kept in, when placed under a cornice or projection, and the absence of sunshine, have a bad effect upon our sandstones. And the fact is sufficiently apparent to any one who will give the slightest attention in passing along the streets; and I may mention in connection with this that sandstones having much clay in them absorb moisture rapidly from the atmosphere, so that such stones are not only longer in drying, but a damp atmosphere will moisten them nearly as much as if they were in contact with water. Two thin pieces, one having 20 per cent. of clay, the other none, exposed to a damp atmosphere over night, the former had absorbed 5 per cent. of its weight of water, the other only 1. This tendency to absorb and retain water may facilitate their decay.

I do not as yet offer any definite solution of the chemical cause of the rapid corrosion of some sandstones. My experiments, however, excite suspicions that the clay which is only mechanically diffused through the stone, has something to do with preventing the adhesion of the siliceous

* Since making these experiments, I have tried the following specimens. Two pieces of stone from Blair Logie Old Church, which had decayed regularly, but not rapidly, gave—

No. 1.	Silica and mica, ...80·5	No. 2.	Silica and mica, ...82·3
	Clay, ...15·5		Clay, ...11·0
	Peroxide iron, ... 1·6		Peroxide iron, ... 2·7
	Carbonate lime, ... 2·4		Lime, ... 2·6
	100·0		Magnesia, ...trace.
			98·6

No. 2 was finer in the grain than No. 1.

A piece from an old castle near Stirling that had stood well; the surface of the stone from which the sample was taken seemed little worn, gave—

Silica and mica,	90·5
Clay,	4·6
Iron oxide,	2·0
Lime,	2·6
	97·0

Looking at this specimen through a lens the silica was transparent and washed like.

particles where there is a paucity of lime, and may be less or more deleterious, from the source of the clay and the position of the stone in the strata, that is, whether the clay has infiltrated into the stone from water passing from the surface, or whether from the felspar in the fine *debris* of granite decomposing, leaving alumina or clay, which is an ordinary occurrence. As to the position of the stone in the seam the density increases as we go down. A cubic inch 2 feet down weighed 19½ drachms, and a cubic inch 8 feet down weighed 22 drachms. The only remedy against decay I am aware being occasionally tried is to saturate the surface of the stone with boiled oil. When care is taken to have the stone thoroughly dry previous to adding the oil, this remedy has been very successful, but it is not the most convenient. If the cause of the decay be found to be a paucity of lime and too much clay, then a simple remedy may be found in drying the stone, which could easily be done by means of heat, and then saturating it with the milk of lime, by which means the surface to a considerable depth would be protected, and the appearance of the stone improved. When a bad stone is placed in ruble work, where it is surrounded by a layer of lime, it will be observed that for about one inch round the stone where the lime has penetrated it there is no corrosion, and the stone compact, and lighter in colour. There are some stones that are found to effloresce in dry weather, and become damp in moist weather, which is no doubt caused by chemical decomposition of some ingredient in the stone; but this is not the characteristic of the decay I am referring to, which is a mere mouldering down, or loosening of the sand particles. The efflorescence of stone is distinct, and may be caused by the presence of sulphuret of iron, which will have that effect, and which I intend to consider on some other occasion on damp walls.

In these experiments there is one thing worthy of remark, namely, the great quantity of water which the stone contains. It is said to require years before a house is properly dry, but it will be seen that this will depend much upon the quality of the stone; but even under the best circumstances when it is considered that every cubic foot of the stone contains at least one gallon water, it will give some idea of the enormous quantity that has to be evaporated before a house be thoroughly dry, but it is also seen that to be thoroughly dry is almost impossible, as the stones absorb water from the atmosphere, more especially if the stone has much clay in its composition. But independent of this absorption from the air, there is also a great quantity taken up by capillary attraction, more indeed than by plunging it into water; taking a cubic foot of stone perfectly dry, it will absorb about one-sixth part more by placing one part in water and allowing it to rise by capillary attraction, than to plunge it overhead in water, the air in the latter case not easily escaping. The presence of clay in the stone makes this attraction more rapid, but does not increase the quantity that a stone will absorb, as the following table will illustrate:—

Per centage of Clay.	Water absorbed per cubic foot of stone.
14 per cent.	1·8 gallon.
17 —	1·5 —
18 —	1·3 —
11½ —	1·4 —
8 —	1·3 —
20 —	1·7 —
13 —	1·0 —
14 —	·7 —
15 —	·8 —
20 —	1·0 —
15 —	1·0 —

Being an average of nearly one gallon and a quarter of water per cubic foot of stone.

The time necessary for the water to rise up from the foundation by capillary attraction is, however, considerable. And it varies much with different qualities of stone, depending a good deal upon the physical structure of the stone. I have witnessed the water rising up one portion of a stone in less than half the time it did in another portion of the same stone, and these different parts by testing give no chemical difference. Taking a 6-inch cube, the following rate is an average of that I obtained; the lower part of the stone was merely allowed to touch the water and did not dip into it above an eighth part of an inch. Taking—

1 minute	to the	1st inch,	it required,
3	—	2d	—
6	—	3d	—
10	—	4th	—
20	—	5th	—
35	—	6th.	
75 minutes	to rise	6 inches.	

This is no doubt a slow rate when we come to several feet, still from a damp found it is certain, and the only limit to it is when the evaporation from the surface is equal to the supply by absorption, and this in damp weather takes it often above one storey of a building; it is no doubt increased by the absorption from the weather outside, and this often to such an extent that I have seen stones inside of a house, two storeys up, become wet, and literally sweat from the water filtering through them. We believe the only preventive yet tried for the capillary attraction rising from the found, is a layer of Caithness slate; common slate I have found equal, if not superior, to Caithness, and cheaper; but whichever be used it ought to be laid above the surface of the ground, otherwise the use of such precautions is neutralised.

Into the subject of damp walls in general, it is not my intention in the meantime to enter, as that may be better taken up separately. I may mention, however, in connection with this subject, that bricks are generally as absorbent as stone. The average absorption in those I have tried is about 10 per cent. of their weight.

I have now only to refer to one or two other characteristics of our building stones, arising in a great measure, I think, from the clay they contain. First, the rapidity with which they become black by exposure. The cause is no doubt the smoke, but the clay in the stone makes this colouring much more rapid from the great attraction which alumina has for colouring and organic matters. A house built of such stone as Craigleith, and one with a sandstone containing 20 per cent. of clay in the same locality, the one would become much sooner coloured than the other.

Another tendency which sandstone has is to become covered with a vegetable growth, and the question has often been put where the seeds of such vegetable matter may have come from. Now, there could hardly be a better condition for the growth of vegetable mould than a sandstone having diffused through it mechanically from 15 to 20 per cent. of clay, with a little lime and magnesia, and capable of absorbing within a few hours several gallons of the moisture of a soil, and no doubt the sporules or seeds of such vegetables either in soils or floating about the atmosphere, are either drawn up by and with the water from the soil, or they attach themselves to the surface of the stone, and find there ample means for their development.

Such, then, are a few experiments and observations upon a subject, I think, deserving a more thorough investigation. I have been induced to bring it before the Society in this unfinished state by a strong desire that those who may be interested in this matter, and who have opportunities of furnishing specimens old and new, so that unquestionable data may be obtained, will be kind enough to let me have them, with any particulars they may consider necessary, with a view to bringing the results before the Society at a future time, should health be granted.

Mr. Ure exhibited an improved form of his Model illustrating the Principle of Ventilation.

Minute of Council.

Andersonian University Library, November 3, 1852.

Present, Mr. Crum, Dr. Allen Thomson, Dr. A. K. Young, Mr. Liddell, Dr. R. D. Thomson, Mr. Gourlie, Mr. Bryce, Mr. Harvey, Mr. Cockey, Mr. Murray, Dr. Mitchell.

Dr. Arthur Mitchell, after some explanatory remarks, produced the

following statement, which the Council agreed to record in their minutes, instructing the Secretary to insert it in the next number of the printed Proceedings of the Philosophical Society, and transmit a copy of the publication to M. Bernard, Paris:—

"To the COUNCIL of the GLASGOW PHILOSOPHICAL SOCIETY,

"GENTLEMEN,—My attention having been called to the possibility of misconception as to a paper 'On the Occurrence of Sugar in the Animal Economy,' (read by me to the Glasgow Philosophical Society, on 20th February, 1850, and printed in their proceedings for that session), in consequence of the omission of certain oral statements which I made to the Society, I have to state, that I never claimed, and do not claim, originality for that communication, which I read to the Society as an account of the researches of Bernard and of the experiments which I had repeated in confirmation of them; that I accompanied the reading with such extemporaneous and interspersive remarks as rendered this fully evident at the time (as appears from letters on the subject from the Secretary, Editor of the Transactions, and Dr. Stenhouse, one of the members of Council, which letters are herewith produced); that the paper was printed on request, with a postscript hastily added to supply the place of these oral remarks; and that I regret that the manner in which the paper has been expressed has been such as to lead to misconception.

"I am, Gentlemen,

"Your most obedient servant,

"GLASGOW, 3*d November*, 1852. "ARTHUR MITCHELL."

GLASGOW:
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PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

FIFTY-SECOND SESSION.

November 2, 1853.

THE Fifty-Second Session of the Philosophical Society of Glasgow was opened this evening,—Walter Crum, Esq., President of the Society, in the Chair.

A framed artist's proof of the portrait of the late Dr. Thomas Thomson was presented by Messrs. James M'Clure and Son, for which the Society voted thanks.

The President read papers on the following subjects: viz.,—"On the Acetates and other Compounds of Alumina." (*Recapitulation.*) "Professor Graham's Discoveries on the Diffusion of Gases and of Liquids."

November 16, 1853.—*The* PRESIDENT *in the Chair.*

MR. HENRY HOW, College Laboratory, and Mr. Daniel Fraser, Pharmaceutical Chemist, 113 Buchanan Street, were elected members.

Mr. Liddell, the Treasurer, gave in the following abstract of his Account for Session 1852-53:—

1852.	DR.				
Nov. 1.—To Cash in Union and Savings Banks,	£123	0	10		
1853.					
— Interest on Banks' Accounts,.....	5	0	1		
				£128	0 11
Nov. 1. — Society's Transactions sold,.....				1	2 6
			Carry forward,	£129	3 5

	Brought forward,				£129	3 5
	To Entries of 18 new Members, at 21s. each,.....	£18	18	0		
	— 11 Annual Payments from Original Members, at 5s,.....	2	15	0		
	— 263 Annual Payments at 15s. each,	197	5	0		
					218	18 0
	— Rent from Sabbath School Teachers, for use of Hall,.....				2	10 0
					£350	11 5
1853.	Cr.					
Nov. 1.	—By New Books and Binding,.....				£100	14 10
	— Printing Transactions, Circulars, &c.,.....				22	14 6
	— Repairs on Hall,.....				0	8 0
	— Rent of Hall, One Year,.....	£15	0	0		
	— Fire Insurance,.....	2	16	0		
	— Society's Officer and Clerk,.....	8	2	0		
	— Postages and Delivering Letters,	11	10	2		
	— Stationery, ...	1	11	3		
	— Gas and Candles,.....	1	8	0		
					40	7 5
	— Librarian's Salary, One Year,....	26	0	0		
	— Do. for Poundage Collecting Dues,	6	18	0		
					32	18 0
	— Subscription to Ray Society,.....	1	1	0		
	— Do. to Palæontological Society,..	1	1	0		
					2	2 0
	— Cleaning the Hall,.....				1	14 10
	— Balance—					
	Cash in Union Bank,.....	143	5	9		
	Do. in Savings Bank,.....	6	6	1		
					149	11 10
					£350	11 5

THE PHILOSOPHICAL SOCIETY EXHIBITION FUND.

1852.						
May 15.	—To Balance, as per deposit receipt, from the Corporation of the City of Glasgow,.....				£573	1 6
1853.						
May 15.	— Interest till this date,.....				22	18 5
					£595	19 11

GLASGOW, 31*st October*, 1853.—We have examined the Treasurer's Account, and compared the same with the Vouchers, and find that there are in the Union Bank of Scotland One Hundred and Forty-Three Pounds Five Shillings and Ninepence, and in the Savings Bank Six Pounds Six Shillings and One Penny—together, One Hundred and Forty-Nine Pounds Eleven Shillings and Tenpence sterling—at the Society's credit at this date.

The Treasurer has also exhibited to us a Voucher which he holds for money lent to the Corporation of the City of Glasgow from the proceeds of the Philosophical Society's Exhibition in 1846, with Interest thereon to 15th May last, being Five Hundred and Ninety-Five Pounds Nineteen Shillings and Elevenpence.

THOMAS DAWSON.
MATTHEW P. BELL.

REPORT BY THE TREASURER, 3D NOVEMBER, 1853.

The Furniture and other moveable property of the Society, with exception of printed Books, remain the same as last year, and a Proof Engraving, in gilt frame, of the late Dr. Thomas Thomson, which has been presented by the Publishers. The Funds in the Bank, as shown in the above Account, are £21 more than at the same period last year. Reference is now made to the Librarian's Catalogue for the list of Books, the property of the Society as at this date.

The number of Members at commencement of Session 1852–53,		281
New Members admitted during the Session,		17
		298
From this fall to be deducted from the Roll at commencement of Session 1853-54, viz.,—		
Resigned membership by letter,	9	
In Arrear of Dues for two years, and held as resigned by Law XI.,	13	
By request placed on Non-Resident List, having removed from Glasgow and paid Dues,	5	
In Arrear of Dues for one year, but have left Glasgow for foreign countries, or places unknown,	5	
Dead,	2	
	—	34
On List for 1853-54,		264

In the above-named list of 264 there are 14 Members in Arrear of Dues for one year, but as they all reside in Glasgow or neighbourhood it is hoped that these will be recovered.

Mr. Cockey, the Librarian, gave in a Report on the state of the Library,—from which it appeared that there had been an increase of 130 volumes during the year, the total number being 2200:—

	Volumes in 1852.	Volumes in 1853.
Case No. 1	327	335
„ 2	319	377
„ 3	279	281
„ 4	170	274
„ 5	245	262
„ 6	302	334
„ 7	137	238
„ 8	191	199
	2070	2200

Increase, 140 volumes.

The Society then proceeded to the Fifty-Second Annual Election of Office-Bearers.

Mr. Liddell moved the re-election of Mr. Crum as President. Seconded by Mr. Bryce.

Mr. Murray moved the re-election of Dr. Allen Thomson as Vice-President. Seconded by Mr. Harvey.

Dr. Walker Arnott moved the election of Mr. William Gourlie as the second Vice-President. Seconded by Dr. A. K. Young.

Mr. Robert Blackie moved the re-election of Mr. Liddell as Treasurer —Mr. Cockey as Librarian—and Mr. Hastie and Mr. Keddie as Joint-Secretaries. Seconded by Mr. Smith.

Mr. W. J. Macquorn Rankine proposed a list of five new Members of Council in room of those retiring, and moved the re-election of the remaining Members. Seconded by Professor William Thomson.

The names of the members so nominated having been written on the black board, the Society proceeded to vote by ballot in the ordinary manner; and Mr. Michael Connal and Mr. John Jeffrey were appointed scrutineers of votes.

The scrutineers having retired to examine the vote-papers,—

The President read a paper "On Dr. Thomson's Lives of the Chemists."

Mr. John Thomson, C.E., described the methods of constructing and laying down the Submarine Telegraphic Wire.

The scrutineers afterwards gave in their Report, when the result of the election was found to be as follows:—

President.

Mr. Walter Crum.

Vice-Presidents.

Dr. Allen Thomson. | Mr. William Gourlie.

Treasurer.

Mr. Andrew Liddell.

Joint-Secretaries.

Mr. Alexander Hastie. | Mr. William Keddie.

Librarian.

Mr. William Cockey.

Council.

Mr. James Bryce.	Dr. Strang.
Mr. William Murray.	Professor Wm. Thomson.
Dr. Thomas Anderson.	Mr. John Condie.
Mr. Alexander Harvey.	Mr. John Erskine.
Dr. A. K. Young.	Mr. James Napier.
Mr. J. R. Napier.	Mr. Jas. P. Fraser.

November 30, 1853.—Mr. GOURLIE, Vice-President, *in the Chair.*

MR. ARCHIBALD MORRISON, Commercial and Mathematical Teacher, 57 St. George's Place; Mr. Robert Hill, 41 West George Street; and Mr. John Leslie, Merchant, 68 Hutcheson Street, were elected members.

Professor William Thomson read a paper, giving an account of Cavendish's Electrical Investigations.

December 14, 1853.—*The* PRESIDENT *in the Chair.*

THE following members were elected, viz:—Mr. Thomas Lester, M.E., Heriothill, 137 Stirling's Road; Mr. Thomas Hoey, Engineer, 93 Pitt Street; Mr. George Martin, C.E., 107 Hope Street.

Dr. Anderson read a paper "On the Chemistry of Opium."

Dr. Walker Arnott gave an account of the Diatomaceæ.

January 11, 1854.—*The* PRESIDENT *in the Chair.*

MR. JOHN KENNEDY, 146 Buchanan Street, and Mr. James Anderson, 36 Surrey Street, were elected members.

Dr. Walker Arnott continued his account of the Diatomaceæ; and Dr. Allen Thomson described some of the lower tribes of the Animal Kingdom.

January 25, 1854.—*The* PRESIDENT *in the Chair.*

DR. DRUMMOND, 80 Bath Street, was elected a member.

The President called the attention of the Society to the improved arrangement of the Seats in the Hall, and proposed that a grant be voted to the Council out of the funds for the purpose of increasing the comfort of the seats and introducing a plan of lighting better fitted for the exhibition of pictures and diagrams.

Mr. Cockey, the Librarian, took occasion to mention that it would be necessary to procure an additional Book-case without delay. He proposed that the sum of £40 be voted for this purpose, and for completing the improvement of the Hall.

Mr. William Brown having seconded the proposal, and recommended that the Seats should be furnished with Cushions,

The Society voted, for the first time, that a sum of £40 be granted to defray the expense of Book-case and improvements.

Allen Thomson made a communication on some of the most recent discoveries in Embryology.

February 8, 1854.—*The* PRESIDENT *in the Chair.*

MR. ANTHONY INGLIS, Whitehall Foundry, Anderston, and Mr. Alexander Mackenzie, Upholsterer, Buchanan Street, were elected members.

A vote of £40 for a new Book-case, Seat-cushions, and Gas-fittings, was unanimously agreed to for the second time.

Mr. Andrew Liddell moved that the Council be authorized to invite the British Association to visit Glasgow in 1855, and stated that if this was done he had reason to believe that the invitation would be cordially accepted.

This motion was seconded by Mr. William Murray, and unanimously agreed to.

Mr. Bryce gave an account of the recent Geographical discoveries in Africa, after which Mr. Alexander Ferguson was requested to prepare and lay before the Society an account of his visit to Egypt and the Nile.

A letter to the Secretary, from Mr. W. J. Macquorn Rankine, was read, calling attention to the circumstance of the Town Council having acquiesced in the limitation of the Ordnance Survey to a scale of five feet to one mile, instead of a scale of ten feet, in favour of which the Society formerly memorialized the Lords of Her Majesty's Treasury.

Bailie Harvey and Dr. Aitken explained, on the part of the Town Council, that the Council would be glad to have the Maps laid down on a ten feet scale, but that Government insisted that the City should pay the difference of expense (estimated at about £2000) betwixt the five and the ten feet scale, which the Council unanimously refused to do.

After a discussion, in which Mr. Bartholomew, Mr. Liddell, Dr. Watson, and others took part, Mr. Liddell moved that the Society should again memorialize the Lords of Her Majesty's Treasury to the same effect as before, which was seconded by Bailie Harvey, and agreed to.

Mr. Murray moved that the Society should memorialize the Town Council also to reconsider the matter, and endeavour to make some arrangement to secure a ten feet scale; seconded by Mr. Liddell and agreed to.

A Committee, consisting of Professor W. Thomson, Mr. Liddell, Mr. Bartholomew, Mr. M'Kain, Mr. Bryce, and Mr. Rankine, were appointed to carry these resolutions into effect.

February 22, 1854.—WILLIAM GOURLIE, Esq., Vice-President, *in the Chair.*

MR. BARTHOLOMEW was appointed Convener of the Committee on the Ordnance Survey, appointed at last meeting.

Mr. William Murray having taken the Chair,

Mr. Gourlie read an account of a short visit to Switzerland, &c., in last September, illustrated by Drawings and specimens of Plants.

March 8, 1854.—*The* PRESIDENT *in the Chair.*

MR. ROBERT GARDINER was elected a member.

The Committee on the Ordnance Survey gave in the following draft of Memorial to the Lords of Her Majesty's Treasury, and, *mutatis mutandis*, to the Lord Provost, Magistrates, and Town Council of Glasgow, in favour of the Ordnance Survey of the city being laid down to a scale of ten feet to one mile:—

"*To the Right Honourable the Lords Commissioners of Her Majesty's Treasury.*

"The Humble Memorial of THE PHILOSOPHICAL SOCIETY OF GLASGOW,

Sheweth—

"That your Memorialists feel a deep interest in the completion and publication of the Ordnance Survey of the City of Glasgow, as being essential to the easy, efficient, and economical planning and execution of improvements in the said city, whether public or private.

"That your Memorialists are satisfied that it has been proved by experience, that in cities such as Glasgow, where the ground is intricately and minutely subdivided, and of great value, and where the buildings and underground works are of a very complicated character, no scale is sufficient for such purposes which is much less than ten feet to one mile.

"That your Memorialists beg leave to express their satisfaction at the intention which they have been given to understand is at present entertained by your Lordships, of causing the Survey of the City of Glasgow to be laid down to a scale of ten feet to one mile, or one nearly approximating to that ratio, and their conviction, that any immediate saving which might accrue from a departure from that intention, would be immensely exceeded by the ultimate loss which would arise from the additional expense and inconvenience in planning and executing improvements."

[*The following clause was added to the Memorial to the Town Council.*]

"That in these circumstances, your Memorialists would respectfully express their hope, that the Lord Provost, Magistrates, and Town Council will reconsider their former Resolution, and give their support to a survey upon the scale of ten feet to one mile."

The Society unanimously agreed to adopt the draft of the Memorial, directing the copy to the Lords of the Treasury to be confided to

Mr. Alexander Hastie, M.P., and requesting Mr. Murray to present the Memorial to the Town Council, and Dr. Aitken and Bailie Harvey to support its prayer.

Mr. Bryce described, with the aid of various maps prepared for the occasion, the geography of the Seat of War.

March 22, 1854.—*The* PRESIDENT *in the Chair.*

MR. ROBERT GARDINER was admitted a member.

Mr. Cockey, the Librarian, laid on the table, No. 7 of the Proceedings of the Literary and Philosophical Society of Liverpool, presented by the Society. Thanks voted.

Mr. Alexander A. Fergusson read "Notes of a Two Months' Sojourn in Egypt."

April 5, 1854.—*The* PRESIDENT *in the Chair.*

MR. WALLACE read a notice "On Red Prussiate of Potash."

Mr. Cockey read a paper "On a Decimal Coinage.'

April 19, 1854.—*The* PRESIDENT *in the Chair.*

IT was agreed, on the recommendation of the Council, that an extra meeting of the Society should be held on the 3d of May, to overtake the arrears of business.

Mr. Macadam read "Observations on the Comparative Purity of Waters in use for Domestic and Manufacturing Purposes in different parts of Scotland; with recent Analyses of the Mineral Wells of Moffat."

Mr. Wallace read a paper "On the Oxidizing power of Red Prussiate of Potash;" after which he described the following

Volumetrical method for the Estimation of Yellow Prussiate of Potash.

The following process was proposed some years ago, but was not considered of sufficient general interest or importance for publication in a separate form. The author was led to search for such a process, in consequence of having been requested by an extensive manufacturer of the salt in question, to find a simple and available method of testing what are technically called the *prussiate cakes.*

The process is thus conducted. 100 grains of the salt are dissolved in two ounces of cold water and a quarter of an ounce of concentrated hydrochloric acid is added to the solution. An alkalimeter of one hun-

dred measures is made up in the usual manner with 21·4 grains of pure and dry bichromate of potash. The chromate solution is then added to that of the yellow prussiate until a blue tint is no longer produced with a solution of perchloride of iron, spotted on a white slab. The number of divisions consumed multiplied by two, gives the per centage amount of pure crystallized yellow prussiate.

The author has not yet been able to explain the action in a satisfactory manner. Red prussiate is undoubtedly one of the products (for it can be obtained in crystals by evaporation), yet the amount of chromic acid consumed by experiment, shows that the action is more complex, and that some other compound is likewise formed. When a caustic alkali is added in slight excess to the mixture obtained as above, the chromic acid and yellow prussiate are reproduced, so that the oxide of chromium cannot be separated by this means. From numerous and careful experiments, it was found that 100 grains of crystallized yellow prussiate are invariably decomposed by 10·7 grains of bichromate of potash.

When the "prussiate cake," or a liquid containing alkaline sulphide, is the subject of experiment, a modification of the process is required, but it is not considered necessary to enter into the details in the present communication.

May 3, 1854 (the Concluding Meeting of the Session was held this evening).—*The* PRESIDENT *in the Chair.*

THE Council was authorized to appoint in due time delegates to attend the next meeting of British Association in Liverpool, and present the Society's invitation to visit Glasgow in 1855.

Mr. J. Napier then read a paper on—

DAMP WALLS.

In the paper I had the honour of reading to the Society last session upon Sandstones used for building purposes, I stated my intention of bringing the subject of damp walls in houses under your notice. The subject of damp walls was so closely connected with the investigations in last paper that I could not avoid having my attention specially drawn to it, and I therefore resolved, from the importance and probable bearing upon the health of the community, or, at all events, the health of those who are living in damp houses, to devote a little time to its consideration. And in communicating to you the results of these inquiries, I will do so in the order in which I conducted the inquiry, first, by considering the nature of the materials used in the construction of houses, and second, the question of damp walls. The materials used in building

may be stated shortly, as *stones, bricks, and mortar.* Stones employed for building purposes may be comprised under two sorts, sandstone and limestone; trap and granite are not in general use for this purpose. Where sandstone can be had plentifully it is most generally used; nevertheless many erections are built by preference of limestone; but the choice obviously depends upon which is most easily and cheaply obtainable. In and around Glasgow sandstone is universally used.

In my former paper I showed that sandstones differ materially both in their physical and chemical structure, and in nothing more than in their power of absorbing, retaining, and giving out moisture. As for instance, stones containing much clay become sooner wet when exposed to a damp atmosphere than those having little clay: that two stones containing different quantities of clay being equally wet, *that* having the least clay will be soonest dry when exposed to a dry atmosphere: that sandstone brought into contact with water, absorbs it rapidly, and in great quantity; and that if only a small part of a stone be in contact with water, it is soon diffused through the stone by capillary attraction; nay, if placed upon a wet substance, such as clay or wet earth, it will absorb the water from these freely, but this capillary attraction differs in rapidity with the structure of the stone. The rate at which this takes place I have made the subject of a few laboratory experiments, most of which have been borne out by observations made upon houses during last winter. The first experiment was with three pieces of stone of different structure, one fine grained, the other coarser, and the third still coarser, all from one quarry, and having about equal quantities of clay. The stones were dried and laid in water to the depth of half an inch, there being 4 inches above the water; the water rose in these at the following rate:—

	1st Inch.	2d Inch.	3d Inch.	4th Inch.	Total.
Fine Grain, ...	17 min.	51 min.	72 min.	127 min.	267 = 4¼ hours.
Coarse Grain, ...	12 ...	47 ...	70 ...	100 ...	229 = 3¾ ...
Coarsest Grain,.....	6 ...	17 ...	38 ...	63 ...	124 = 2 ...

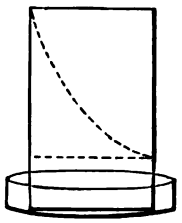
Here there is a vast difference in absorption. The next trial was with two pieces of equal fineness from different quarries, the one had 14 per cent. of clay the other 12 per cent. These were treated in the same manner, but there were seven inches above the water.

	1st In.	2d In.	3d In.	4th In.	5th In.	6th In.	7th In.	Total.	Hours.
No. 1 Time,...	21	88	85	165	170	150	180	854	14¼
No. 2 Time,...	17	60	65	130	145	230	263	910	15

From these it appears to be the physical structure that causes different rates of absorption. The rate of progression we think worthy of further notice.

A piece of Craigleith stone, of the sort referred to in my last paper, was tried along with these, but the water did not rise one inch in twelve hours.

Placing one of these stones in the position above described, the dimensions of the stone being seven inches high and six inches thick, and dropping upon one face of the stone a little water so as to imitate a shower of rain falling upon the walls of a house, the stone very speedily becomes wet all over, the capillary attraction seems increased, and the absorption goes on somewhat like the dotted line in the following figure:



In this experiment the stone was wet all over in less than half the time required to do so by capillary attraction alone, so that when a house is built upon a wet foundation, the water will rise up the walls by capillary attraction, and during rain this rise will be greatly increased.

The height to which the water will rise in the wall of a house will depend somewhat on the state of the atmosphere. When the evaporation from the surface of the wall is equal to that drawn up by capillary attraction, that will form the point to which it will ascend; but this is by no means a small height. I have observed in a good many houses built within these five years, where the stone is not yet so black as to hide the damp, that the line of damp has reached from five feet to seven feet above the surface of the ground, and upon pillars at the sides of doors, the water generally rises in these from one to two feet above that of the ashlar wall, no doubt from its being one stone, showing how putty between stones helps to retard the rise of the water. Where the found has been laid upon sand or gravel the wet does not ascend so high by nearly two feet. The whole length of time required for the water to ascend the height of seven feet I have not positively ascertained, but, when once it is up a wall, the drying is a slow process. In two instances that I have strictly watched, the wet did not descend more than six inches during six weeks dry weather.

I would suggest a very simple remedy against the capillary attraction from the foundation, by putting one or two courses of common house

slate embedded in Roman cement above the surface of the ground. Were one layer put below, and another above the base course, no damp would penetrate them.

Limestone

differs entirely from sandstone in its constitution: the latter is a rock composed of siliceous particles cemented together by lime under pressure. Limestone is a mineral chemically combined; nevertheless its physical structure is exceedingly varied, including from the marl to the marble. It in general absorbs water much more slowly than sandstone, and the capillary attraction is very slow, requiring, according to several experiments I made with compact limestone, several hours to ascend one inch; nevertheless, although requiring more time to become wet, it in many instances absorbs more water than some sandstones, and is much longer in drying. I have not had the same opportunity of experimenting or making observations upon limestones used for building purposes as upon sandstones, and must therefore refer to the experiments and observations of others. The report of the commissioners upon building stones for the New Houses of Parliament supplies a good deal of information upon this question; and from that report it appears, that limestone, when thoroughly dry, and then put into water, absorbs a great quantity of it, varying from half a gallon to two gallons per cubic foot of stone, equal to that which I obtained in the sandstone. It may be stated in general terms, that the oolite and magnesian limestones are most absorbent, averaging $1\frac{1}{2}$ gallon per cubic foot of stone, and the common compact limestone least absorbent, averaging not more than $\frac{3}{4}$ of a gallon per cubic foot of stone. Analyzing the same class of limestones that differ in this absorbing property, I found nothing to suggest a chemical cause for the difference, and think it depends upon the physical structure.

The slowness with which lime absorbs water is a feature I wish to be remembered, as it has a peculiar bearing on damp walls to be afterwards noticed.

Bricks.

The next building materials to be considered are bricks. I need hardly say that they vary much in their character, as the most casual observer cannot fail to notice; both their physical and chemical constitution, their property of absorbing moisture, and the rapidity with which they do so varying very greatly. This, however, depends much upon their baking. When the brick has been so situated in the kiln or oven, that a partial glaze has been formed upon the surface, such bricks absorb little water, and slowly and almost wholly by parts not so glazed According to a number of experiments made upon different qualities of bricks, I found them to absorb from 5 to 13 per cent. of their weight of water, being

from ¾ to 2 gallons per cubic foot; and these experiments were made with what are termed good outside bricks, there being often used in building partitions, soft, coarse bricks, which imbibe water like a sponge. In some trials the water passed up the whole length of a brick by capillary attraction in eight hours; in other bricks it took twenty hours, which may be stated as the two extremes. The absorption and ascension of the water in well burned and partially glazed bricks, was often confined to one or two faces of the brick that had escaped the glazing; so that, if means could be adopted to partially glaze the whole surface of the brick, the absorbent property would be removed. The application of a glaze to bricks I am afraid would not do, the low heat at which it would require to fuse would render such glaze liable to decompose in the air. Bricks, from the poverty of the clay used, fuse at a low heat, so that I think a different mode of baking or firing so as to produce the partial fusion over the whole surface of the brick that is now confined only to one or two faces, would do much to improve the quality of brick, so far as the protection from damp is concerned, although it would be against their binding by lime, as a certain amount of absorption is necessary for this purpose.

Plaster or Mortar.

The next substance to be considered is plaster. If a little plaster be taken after hardening and drying, and put into water, or if water is allowed to drop upon it till saturated, it takes up fully two gallons per cubic foot. Its capillary attraction is very rapid for two or three inches, after which it becomes slower than the average of brick or sandstone. It gives out any excess of water easily when brought into contact with other absorbents in a drier state, thus evaporating and communicating moisture freely to everything around it. Plaster, however, must be considered in its different ingredients, lime, sand, and water, all of which play important parts in damp walls.

Lime used for mortar differs altogether from the limestones used in building, having been subjected to the operation of burning, where its property is altogether changed, and then to slacking, when it absorbs or combines with from one-third to one-half of its weight of water. If the limestone has contained any acid or alkaline salt, which is often the case, these are affected by the heat and slacking. If the water contains any salts or acids, which is universally the case, then there will be formed in the mortar ingredients subject to decomposition, and also, which is not uncommon, the sand mixed with the lime contains matters subject to decompose, such as common salt or organic matter, all of which in a short time become a source of perpetual annoyance, showing itself either in the form of damp or efflorescence upon the plaster, which I will notice presently. Such, then, are the principal materials of our walls, which

are likely to produce damp. Wood, no doubt, forms an important item in the building of a house, but wood is generally seasoned before being put into a building, so that, of itself, it is not a source of damp. Nevertheless, new wood absorbs moisture rapidly, either from a damp atmosphere, or when in contact with wet substances; so that the wood about a house, from contact with damp, very soon becomes a source of moisture in newly-built houses, and that it is no mean source of supply by increasing the evaporating surface, will be evident when I mention that it will absorb from a damp wall fifteen per cent. of its whole weight of water, and give it out very easily by evaporation or contact, as may be amply verified in wall presses of newly-built or otherwise damp houses.

We have now to consider the general question of damp walls, both in new and old houses; and, being guided by the data obtained, let us first consider the state of a new house finished for a tenant, both those built for the labouring classes and for the middle classes.

The stones naturally contain, on an average, $1\frac{1}{4}$ gallon per cubic foot. They never lose all this water, retaining about $\frac{1}{8}$ even under the most favourable circumstances for drying. The quantity they lose the first year varies according to these circumstances; but under the most favourable conditions, and where there was good ventilation, according to one experiment, the loss amounted to fully half a gallon per cubic foot.

The bricks used for partitions, although not wet of themselves, become saturated by the mortar and plaster put upon them, and therefore may be said to contain $1\frac{1}{4}$ gallon per cubic foot.

The plaster in a condition fit to work I have found by several trials to be by weight—

45..... sand,
5..... lime,
50..... water,

so that one hod of this contains about 5 gallons water, which go over but a small surface of wall, being sufficient to supply either brick or stones not previously saturated. After a few weeks in dry weather the plaster hardens and seems to sweat, probably from taking up carbonic acid. When the excess of this sweating is evaporated, and the plaster perfectly hard, the house is, in general, ready to be occupied, more especially those for the working classes. I have often seen such houses tenanted before this, and before the plaster had properly hardened, and when it could not contain less than 20 to 30 per cent. of water—a practice most reprehensive, and against which the law should be enforced upon landlords. Workmen's houses have no lining or battening, but are plastered upon the stone walls, so that when they are considered fit for occupation the walls must contain about $1\frac{1}{4}$ gallon per cubic foot. So that if we take a room 12 feet square and 10 high, and if only

two inches of the depth of the wall lose half this moisture the first year, then there will be upwards of 100 gallons water evaporated in a house unprovided with proper means of ventilation, subjecting the tenants to an incipient vapour bath, in which are dissolved the noxious gases generated in the family. Clothing in such an atmosphere also imbibes moisture, and becomes damp, so that to contemplate the whole circumstances only increases surprise that fevers, consumptions, and other diseases are not more prevalent, and instead of the rate of life of that class being 20 years less than the upper classes, the wonder is that they can rear their own offspring at all.

The new houses built for the wealthier class, are constructed so that the damp is not so great during the first and second years. The stone walls are battened, leaving a stratum of air between the wall and plaster, so that the plaster dries sooner. Brick partitions are however in the same state as common houses, the plaster being put on the wall. Such houses are all papered, which renders the evaporation less apparent and slower, but withal the plaster absorbs the moisture from the walls; notwithstanding the very thin stratum of air intervening, the paper and all upon it absorb damp and evaporate freely, except those parts covered, as will be manifested by mould and damp under places where pictures hang close upon new walls, and this constant evaporation is not good, especially in sleeping apartments. The damp causes decomposition of paste and paper, producing a bad smell, and a very unhealthy atmosphere. The damp walls I have just described originate from the natural moisture in the materials used, and are cured by time as the matters dry. But there is a class of damp walls very common and destructive to health, which no time cures. These are mostly in first floors of houses, and originate from the foundation and the absorbing property of the materials. After the drought of summer these walls become comparatively dry, but whenever wet weather sets in, immediately the damp begins at the floor and ascends gradually until it reaches from four to six feet high, and there it comes and goes for several inches up or down according to the weather, until the return of the summer. In the working man's house this is visible by a dark line along the plaster, and we have often seen that line fringed by a slight efflorescence. Such apartments always feel cold. In the winter, fires are increased, and so is the moisture, producing to an unlimited extent all the evils I have referred to in new houses. In a papered house the evils are the same as formerly described, although not so great as on walls without paper, except smell. In a short time the paper loses its attachment to the plaster, and falls off. Canvas is often used under the paper to prevent the loosening of it, but that will not prevent the evils of damp. We have already referred to layers of slate being put above the found as a good preventive

of this kind of damp walls. I have been informed of a mixture of lime with oxide of iron, and a certain kind of ashes ground, which soon hardens so as to become impervious to moisture. I have not seen the mixture, nor houses where it was used, but, if effective, it is to be regretted that it is not more generally employed.

Damp also penetrates through the side walls of a house, from the thinness of the wall and the porous nature of the stone. I have several times witnessed this on the second floor of a three and four storey house so great as to loosen the paper and be visible upon the plaster inside, and to appear in every continuance of wet weather. I believe that this is a very prevalent source of damp in houses exposed to the weather, although it may not be so great as to attract notice. Now prevention from this source could be effected cheaply and easily. I have seen houses on the coast tastefully painted over by a sort of pitch and tar, and have often considered it a lesson that might be applied beneficially to the outside of our porous walls, and any colour could be given to such cheap matters as would throw off and resist the wet penetrating the stone or brick.

Houses under the level of the ground, and especially where the ground abuts upon the wall, are never dry, and should never be inhabited by any person, and especially never used as sleeping apartments.

There is another cause of damp on walls, which affects a certain class of houses very much, such as those having bare walls. This is caused by sudden changes of temperature, and moisture in the air. When the temperature has been cold for some time, and suddenly becomes warm, the plaster and stone being bad conductors of heat, remain for a long time much colder than the atmosphere. The consequence is, a condensation of the watery vapour of the air, and in great quantities, for the condensation sets up a current of air towards the wall, and the water so condensed is often very considerable. If the walls be of non or slow absorbent materials, the moisture collects and runs down in drops, and attracts notice, and should be removed by a cloth or sponge. Limestone walls are very subject to this from their slow absorbent property; hence people get an impression that limestones are more liable to damp than sandstone, while of all kinds of damp this is the least to be dreaded, it being easily removed. Lighting a fire in a room upon a cold day will produce the same effect upon such walls, and give rise to the common notion that the fire is drawing out the damp. The same condensation takes place upon plaster and sandstone walls, but the moisture is absorbed and does not become visible. We have seen a dry plaster under these circumstances become saturated with moisture, and penetrating to the depth of two inches, and the room being damp in consequence for weeks after, and this is a condition and result always occurring, especially in spring-time and harvest. Papered walls are affected in the same way, but probably *not to* the same extent; but this damp imbibed and retained in a room

where there is seldom a fire kindled and little ventilation may cause decomposition of the paste and paper, and produce other evils. At all events, paper upon damp walls always has a heavy disagreeable smell.

Another and very annoying kind of damp occasionally seen upon walls, is caused by certain ingredients in the stone, brick, or plaster, undergoing decomposition. In dry weather the wall seems as if covered by hoar-frost, owing to an efflorescence, and in wet or damp weather this either disappears or seems to run into water, and the wall becomes damp. In other words this efflorescence is very soluble, and when the moisture becomes condensed on the walls, as we have just noticed, if the wall be absorbent, the whole seems to disappear; but if of limestone or other slow absorbent substance, the wall becomes wet. This efflorescence upon walls depends, as I have already stated, upon the presence of certain salts in or upon the materials forming the wall. Sandstones occasionally contain sulphuret of iron, which by exposure to air and moisture becomes oxidized, the sulphur being converted into sulphuric acid, which readily combines with some ingredient in the stone, and forms salts, soluble in water, and soon affects the surface of the stone, as described as a fine hoar frost, mixed with sand from the disintegration of the siliceous particles of the stone. Bricks often effloresce from the same cause from the clay or fuel containing iron pyrites, which, during burning, if the heat be not very high, becomes converted into sulphuric acid, which readily combines with one of the ingredients in the clay, and forms salts which are dissolved by the moisture absorbed by the brick, and ultimately brought to the surface. The plaster also is subject to effloresce from the lime, water, or sand containing matters that will form soluble salts. As an illustration, I will suppose the plaster made from the waters of the city wells. According to Dr. R. D. Thomson, the acid in these wells, taking the average, stands thus in each gallon:—

18 grains sulphuric acid,
10 grains muriatic acid,
2·7 grains nitric acid,

in all 25·7 grains,

either in combination with bases, or subject to combine with bases, and form salts, giving at least twice its weight, or 67 grains per gallon. Now every 100 pounds of plaster, as put on the walls, contains 50 lbs. or 5 gallons water, having in it 255 grains soluble salts. This no doubt seems, and really is, exceedingly little, when diffused through 100 lbs. plaster, covering about one yard of wall; but it does not remain diffused through the plaster, but is brought to the surface by the water which on being evaporated leaves the solid salt exposed. It is therefore of importance to pay attention to the quality of the water used in mixing lime,

and should the water or any of the substances used have organic matters in them, these, with damp, are subject to decay, and produce salts that will affect a wall.

The analyses of these efflorescences have verified these statements. The white coating upon bricks has been found to contain sulphuric acid, alumina, magnesia, soda, and iron. Upon a brick wall near to where I live, one side of the wall seems whitened over, leaving, generally, the centre portion of the brick. I collected some of this, which was a fine powder, resembling effloresced soda. This was tested, and gave sulphuric acid with a small proportion of hydrochloric acid, no alumina or iron, but magnesia, soda, and a trace of lime—all which I consider to come from the lime between the bricks, which had been absorbed; and the true source, the water used in mixing the lime when building. Kuhlmann and Vogel have analyzed efflorescence upon walls, and give as their general composition sulphate of soda and potash, carbonate of soda and potash, and chloride of sodium and potassium, and their opinion is, that these salts are from the lime, taking no notice of the water used. Whichever be the source, we have found the composition of the efflorescence to vary according to circumstances. A snowy efflorescence growing out from plaster to the depth of half an inch, a sample of which is exhibited, I found to be almost wholly composed of sulphate of soda, having a mere trace of magnesia, lime, and chlorine. Another sample, upon a different wall, different lime being used, but in the same locality, I found to be of the same composition, and also, that mostly all the spring waters in the locality (Partick) contain soda. In a house built last spring, the walls of which outside were pretty well dried during summer except the two first courses of stone from the ground, they have been damp all along, mostly, in my belief, from the found; but during the late dry weather these stones have become coated over with a white floury efflorescence. I washed off some of this, and found by testing that the water was slightly alkaline, and gave soda, magnesia, and a little lime, with sulphuric and muriatic acids. These salts I believe to be mostly from the water absorbed from the found.

In a house I occupied in Swansea, there had formerly been a rent in the side wall, of nearly two inches wide, which had been filled up by lime. Against this wall a wing had been erected, so that when I occupied it this wall was not exposed to the weather; nevertheless, all along this rent, and for 12 inches of the plaster on each side, no paper would adhere for any length of time. In damp weather it became damp, in dry weather it effloresced; and though brushed off again and again, the source seemed inexhaustible. I collected ten grains of this efflorescence, and submitted it to analysis, and obtained the following, in 100 parts:—

Sulphuric acid . .	33·1
Muriatic acid . . .	5·3
Carbonic acid . . .	14·7
Potash . . .	10·9
Soda . . .	25·3
Magnesia . . .	2·0
Lime	3·3
Water and loss . .	5·4
	100·0

This analysis corresponds somewhat to the circumstances. Swansea is upon the sea-shore, and in the immediate neighbourhood of extensive copper-works, which are constantly sending into the atmosphere sulphurous acid, often enveloping the town in a dense fog, which condenses and deposits during chilly and dewy evenings sufficiently to destroy vegetation. By inquiry I learned that some time before I occupied the house, the guttering along this wall had got out of repair, so that rain was admitted into the wall. Now after a short time of dry and calm weather, the smoke from the copper-works having been over the town, the first rain occurring after, coming off the roofs of houses, always contained sulphate of soda, and very little chlorine, although, no doubt, the soda was from sea-salt that had been decomposed—hence, this no doubt was the source of the efflorescence and damp in this crack. Although this analysis may not represent the general composition of efflorescence on walls, the whole circumstances are suggestive of causes that may produce these effects.

Nitrates in plaster have been long known as a source of efflorescence on walls; and although I found none in the samples I tested, I have no doubt they are often present, being very easily formed when organic matters are present; and, as I have mentioned, such salts may be absent in one locality and prevail in another. Nitrates are always found to prevail upon the walls of stables, &c., no doubt from the soluble organic matters absorbed by the wall or plaster after being built or used as a stable or cowhouse. There is a practice of workmen urinating upon the inside wall of a house being built: this, when continued in one place for any length of time, is reprehensible, and often requires years to get quit of the consequences.

In speaking of the efflorescence and diliquescence upon walls, it is often supposed that these effects are caused only by the presence of such salts that have by themselves efflorescent or diliquescent properties. This is a mistake. Every substance, soluble in water, that is either put in or formed within a wall, produces these effects. They are brought to the surface by the water, and there left dry, giving the appearance of efflorescence; and, when a more moist atmosphere occurs, the salt is again dissolved, giving the appearance of deliquesence and damp.

Such, then, is a brief statement of the several causes and circumstances attending damp walls, which, I think, ought to suggest precautions against these effects, so far as practicable. And were the question of health, comfort, and economy to form an element in the calculation in building a house, the evils referred to should be avoided; but I am afraid that in most of the houses constructed of late, especially for the working classes, such questions are seldom proposed; and should they occur, the fear of half a per cent. less interest is an effective negative.

I wish I could enter into the questions as to what is the maximum of moisture in the air, a healthy person may live in, sleeping and waking, and how far his system can resist the effects of his being surrounded within a few feet by an evaporating surface, absorbing rapidly the heat given off by the body. Such questions may not be easily answered, and probably no fixed standard could be given; but I am afraid that a continuation of these conditions would be at a risk to the constitution of the most healthy. I have, however, endeavoured to give some data to those whose knowledge and means of inquiry may extend to the ascertaining of these important points, which I think of some importance to a community such as ours, when the fact of not being able to pay a rent of £20 or £30 will place a family under circumstances where the average rate of life will be shortened by 20 years, and where the chances are, that the family receive a moral taint difficult to overcome, as the circumstances have a strong tendency to weaken the influence of either moral precept or example.

Mr. J. R. Napier exhibited a Vacuum Gauge for Steam Engines.

Mr. J. Napier also read a paper on "Spurious Coins."

MR. JAMES NAPIER ON SPURIOUS COINS.

The present inquiry into the character of spurious coin, originated with my friend Mr. Arch. M'Laren of the Glasgow Stamp Office, who, in the course of a number of years, had collected a great many, and felt curious to know their general character and if they possessed any intrinsic value. This inquiry, like most others, soon became interesting to us both, and with Mr. M'Laren's consent, I shall now read the results, in which some members of the Society may also be interested.

GOLD COINS.

As may be anticipated, spurious gold coins are not so plentiful as either silver or copper; this may be accounted for by gold being more difficult to imitate in all its general qualities as a coin, than the other metals, and also in much more care and attention being bestowed upon

accepting of gold coin than on those less valuable, especially in Scotland, where gold is comparatively scarce. The only spurious gold coin examined was a half sovereign: it was beautifully finished, and resembled a new coin; but was much lighter, weighing only 43 grains, while the genuine coin should weigh about 61 grains. It was gilt by the electrotype process so perfectly that when the spurious metal was dissolved out, the gold gilding remained as a skeleton coin with all the impressions upon it, and the whole weight of which was only 0·3 of a grain. The spurious metal was a rich brass; the composition of the coin was

Copper,	28·8
Zinc,	13·2
Iron,	0·7
Gold,	0·3
	43·0

This coin was evidently the work of a tradesman, and the use of such a rich brass enabled him to use a thinner coating of gold than would have sufficed for copper or any white coloured metal to obtain the real colour of a gold piece, as the metal shines through a thin coating of gold.

Silver Coins.

The number of spurious silver coins in our possession for examination was considerable, and of the several values from a sixpence to a crown. When a quantity of these spurious coins are put together it is at once apparent that there have been no great variety of materials used in their manufacture; probably from the metal most resembling silver, viz., tin, being abundant, and within easy reach of the coiner, is easily fused and well adapted for casting, and a very little experience teaches what are the metals best adapted to mix with tin for the purpose of coining.

The whole currency of spurious silver is by stealth, and whenever discovered, which is very easily and soon done, the honest possessor, rather than defraud his neighbour, stops them. Yet, when we take into consideration the number constantly being detected, it shows that their manufacture must be in great quantity, and that society suffers severely from this source.

The whole variety of these coins may be divided into three. The first, and not at all a very common coin, and mostly confined to half-crowns and shillings, are made of brass, and then washed over with a solution of tin in the same manner as they whiten pins. This sort very soon become yellow round the edges and prominent parts, stopping their circulation; but even when new they are easily detected by their weight as well as by the sound, the ring being sharper and of shorter duration than silver.

The most of these examined were George III., dated 1819; their average weight was 186 grains. One analyzed gave in the 100 parts—

Copper,	61·0
Zinc,	31·3
Tin,	6·7
Lead,	0·5
Iron,	0·4
Silver,	trace
	99·9

This trace of silver may have been originally in the copper, which often contains that metal. I do not suppose it was applied by the coiner.

The next variety, and by far the most common, are made of commercial tin without any artificial alloy or coating. These are not generally so well got up as the next sort, and are apparently the produce of several hands. Tin is nearly the same colour as silver, is easily melted in an iron ladle or spoon over the fire, and is not subject to tarnish by exposure to the air, qualities fitting the most ignorant coiner,—however, tin coins are easily detected: when compared with silver they have a bluish tint,—the sound, when thrown upon wood, being more of a clink. The metal is soft, so that the coin is easily bent by the teeth, and when bending gives a crackling sound; indeed, this is the most simple and effective test, to give them a bite; even when not bent the teeth makes an impression, and the crackling is felt instantly, and may in this way be as easily detected in the dark as in the light. The weight of these coins is about three-fourths that of silver. The average of the sixpence was 31 grains—the average of the shilling was 57·5—that of the half-crown was 158. They are mostly cast from coins of old date; 1816, 1819, and 1827, were common dates.

The analyses of these several coins gave no more difference than is to be found in different lots of common block tin for sale in the market, so that no alloy seems to be employed in their manufacture. The average composition may be stated as

Tin,	97·7
Iron,	1·1
Lead,	0·8
Copper,	0·4
	100·0

The third class of spurious silver coin is the most improved. They are generally well got up, and are composed of an alloy that in appearance comes closer to silver than pure tin, not being so blue. They are heavier than the tin coin, but still a full fifth lighter than silver, and the

ring is much improved, but still sharper and of shorter duration than silver. They are easily marked by the teeth, and give the crackling sound of tin but slightly.

A shilling of this sort analyzed gave in 100 parts—

Tin,	90
Copper,	10
	100

A crown piece, of which there were recently a good many in circulation, gave in 100 parts—

Tin,	87·4
Copper,	12·3
	99·7

This latter was the best alloy for the purpose. These sort are often cast from models of a late date, and when they are newly made, and before the air has had any effect upon them, they are a most perfect counterfeit so far as appearances go. However, there are many also cast or stamped from coins of old date. The shilling analyzed was dated 1836, and the crown 1820. In general spurious silver are made from partially worn coin, no doubt to take the advantage of the impression that if a coin is worn it is more likely to be good, and a worn coin is not so easily detected by the weight in the hand. As a great deal of our silver coin is very much worn, I have had them nearly one-fourth lighter than a new coin by abrasion. We may remark that there is one general feature in the whole of these silver coins that may be easily taken advantage of where there is any dubiety of their genuineness, namely, the weight. Selecting a good coin of the same wear and putting them against others in the balance, the spurious will be found wanting from one-fifth to one-fourth of the whole weight, when there need be no hesitation in rejecting or retaining the bad article. Or if rubbed a little between the fingers, they have a heavy smell which genuine coin have not got.

The following paragraph has been printed and circulated, showing a new alloy in use for spurious silver coins, and marks a quality I have not yet seen:—

"NEWLY INVENTED SPURIOUS COIN.—Within the last few days Mr. Webster, the Inspector-General of Coins to her Majesty's Mint, has received some counterfeit shillings, bearing date of those issued in 1852, and which more perfectly represent the genuine coin than anything ever yet put in circulation. Their difference from the ordinary bad money is, that they are struck with a beautifully executed die from a hard white metal, which is subsequently strongly electroplated. Their being struck from dies renders them to all appearance perfect in this respect, that the rim and nerling is cut quite sharp and complete, whereas in the

Britannia metal base coin hitherto circulated, that being cast in moulds and poured in from the edge of the mould, the outer rim of the coin is always faulty, and a very cursory inspection of that part would suffice to detect it. The component parts of the metal from which they are struck are copper, nickel, and zinc, in the following proportions,—copper, 64·26 per cent., nickel, 15·71, and the rest zinc; which is, in reality, German silver. The shilling weighs 5 grains light of a genuine one of the same year, and three grains heavy of a George III., of 1822. The metal being so hard, the coin detectors will not expose the fraud, but they may be known by the ring, which is very bad. It is believed that they came from Birmingham."

COPPER COINS.

The copper spurious coins may be divided into four sorts, although there is a great variety of the first two sorts. And, upon the whole, copper coining appears to be the most extensive, probably because there seems to be little or no risk attending it, and the profits, as we shall presently see, are very considerable.

The first and worst sort are composed of brass, and afterwards bronzed by acids to give them the appearance of tarnished copper. This sort does not maintain its circulation, the bronze soon wears off and the brass is detected. This sort I do not think numerous, many of them are often the production of thoughtless youth in brass foundries, although, no doubt, there are many made by the worthless to maintain a life of idleness. The most of those I have seen are cast from Irish pennies. One analyzed was George IV., 1823, and weighed 276 grains, and gave per cent.—

Copper,	59·2
Zinc,	32·9
Tin and antimony,	4·1
Lead,	2·2
Iron,	1·6
	100·0

This, I need hardly say, is inferior brass.

The next sort is a little better, being made of bronze and afterwards darkened by acids. These were mostly all in the form of old penny-pieces, the bronze colour is only visible round the rim; these maintain their circulation, although under suspicions. The one analyzed weighed 323, and gave per cent.—

Copper,	83·5
Tin,	2·1
Zinc,	10·0
Lead,	2·4
Iron,	1·8
	99·8

The next two sorts are composed of good copper, and differ only in the one having been cast and the other stamped by hand. The first of these are not numerous, and mostly moulded from the old penny. They feel light, and have a light sharp sound compared to the genuine coin, and smaller in diameter,—their average weight was 297 grains, fully one-fourth lighter than a good coin of the same wear. The analysis gave

Copper,	98.9
Silver,	0·5
Tin,	0·1
Iron,	0·4
Lead,	trace
	99·9

which is most excellent copper.

The next sort is by far the most numerous, and are composed of equally good metal; but they are a most miserable coin as respects their make, and if found by some future antiquarian numismatist would give him a very poor and unfair impression of such an art in this age,—these are made resembling pennies of various dates, some of them the most recent, and I have no doubt form a very important branch of manufacture to some parties, and as no one objects to take them their manufacture is without risk.

The commercial and political aspect of this state of our copper coinage I do not here consider, but from the information I have obtained I believe there can be no less than from 25 to 30 per cent. of the spurious copper money in constant circulation in this part of the country which no doubt originates in and is maintained by the scarcity of genuine copper coin. From a parliamentary paper recently printed, it appears that only about 330,624 pennies and 455,616 halfpennies have been made yearly these last six years by Government, certainly far too little for the requirements of this country, and the equivalent of this supply not reaching distant places, causes or rather necessitates a far greater and easier circulation of spurious copper coins here than in London, which is a well known fact. The scarcity of copper coinage and the great inducement to parties to manufacture to supply this deficiency will be best stated by showing the profits to be made by such manufacturers. Take copper at the average price these three years back, it does not exceed £100 per ton. Thus

112 lbs. copper cost	£5	0	0
112 lbs. coined at the Mint	11	4	0
112 lbs. made into cast pennies as above	14	12	0
112 lbs. made into stamped pennies as shown	17	0	0

Here is a source of a comfortable living to a few *Brummagem garret men*, and, as we have said, without risk.

Should we take the silver coining in the same way, it seems vastly superior as a commercial speculation,—1 lb. of the tin alloy will not cost more than 1s. 6d., and will produce about 96s. These, however, are often disposed of to venders at three and four for 1s., thus reducing the profits—this, along with the risks of detection, imprisonment, &c., necessitates the silver coiner to belong to the lowest class of society; but both the copper coin and coiners of them maintain their position in society, it only requiring that he keep his trade secret, the name coiner not being legal as a private manufacture.

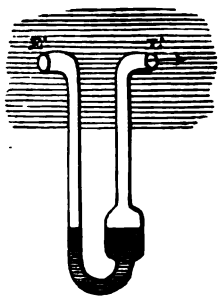
The following is the standard weight of each coin when new:—

Sovereign,	123 grains.
Half-sovereign,	61· —
Crown,	436 —
Half-crown,	218 —
Florin,	174 —
Shilling,	87 —
Sixpence,	43 —
Fourpence,	29 —
Penny,	292 —
Halfpenny,	146 —
Farthing,	73 —

Description of an Instrument for Measuring the Velocity of Ships, Currents, &c. By Mr. JAMES R. NAPIER.

A bent tube with its orifice exposed to the passing water will, by the height to which the water rises in the tube, indicate the velocity of the vessel or current.

Tubes of this description have been tried, but the difficulty of ascertaining the zero point, or the level of the surrounding water from which to measure the height, especially in a boisterous sea and with every varying immersion of the vessel, has hitherto rendered this simple construction unsatisfactory.



In the instrument now submitted, I overcame this difficulty by using

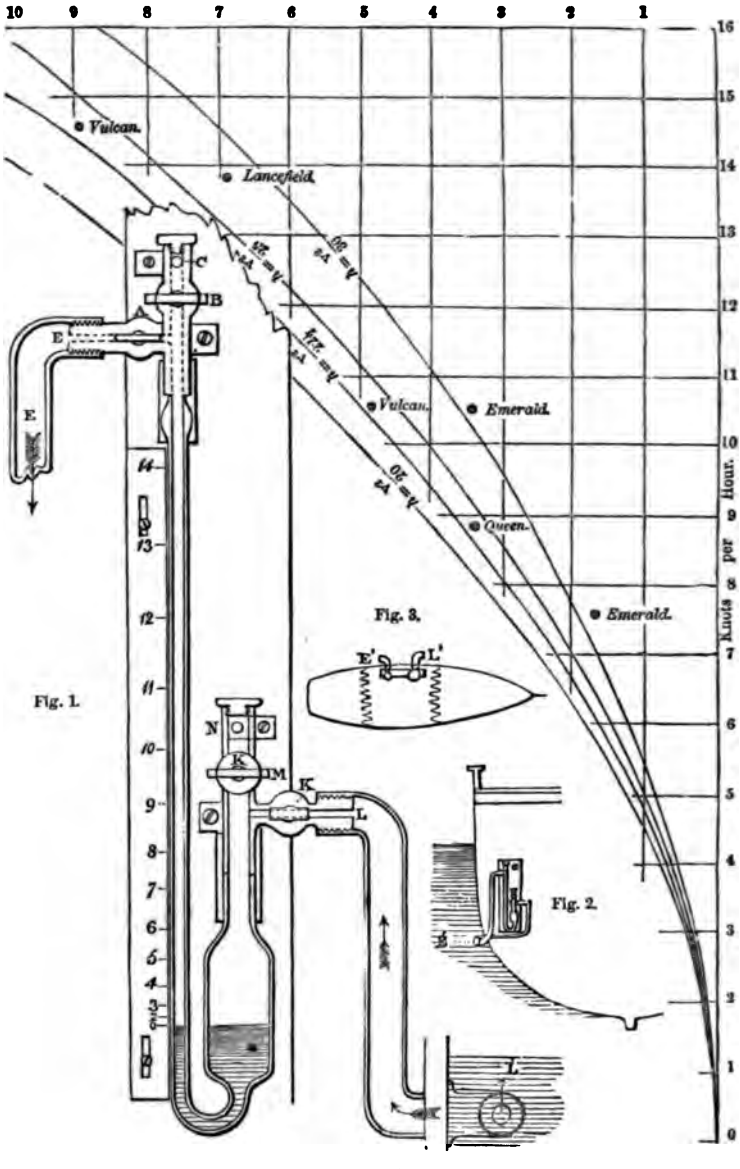
two bent tubes, the one having its orifice looking forward, and the other its orifice looking aft, and their other extremities connected with a bulbed glass tube containing a little mercury.

The velocity of the ship is indicated by the height to which the mercury rises, and, as when the vessel is at rest or moved vertically, the pressures on both the exposed orifices are, and always continue equal, neither the varying immersions nor boisterous seas can have any influence on the heights to which the mercury will rise.

Fig. 1 shows a reduced view of the instruments I have used. The bent pieces L′ and E′ are fastened to the side of the vessel well under water, and, I believe, may be fixed at any part, though I have hitherto had them placed about the middle, in the engine compartment of steamers. The instrument itself, I had thought, might be placed in any situation where it could be most conveniently seen,—as, in the captain's cabin, for instance. I now find that it is most effective when placed below the external water level, as when placed above this level the indications become uncertain from the accumulation of air which then separates from the water. The stop-cocks B and M, and the short tubes at C and N, are for the purpose of allowing air to escape if such is suspected to be present; and the stop-cocks A and K for regulating the size of the orifice, so as to prevent the oscillations of the mercury. The pieces E′ and L′ were connected with the instrument at E and L by block tin and Indian rubber tubing. A scale of tenths of an inch placed alongside the glass tube, with its zero level with the mercury in the bulb, shows the heights to which the mercury rises, when the vessels are propelled at different speeds.

I imagined that the velocity would be indicated by the usual formula $v = n\sqrt{h}$, h being the height of the mercury, and that when v is taken in knots per hour, and h in inches, n would be a constant quantity, if not for all ships and at all velocities, at least for the same ship at all velocities, and, if constant, its value would be nearly 5, found by reducing the formula $v^2 = 2gh$ from feet per second to knots per hour, and to h inches of mercury instead of feet of water. The results I have recorded, however, do not exactly corroborate this; but the experiments are perhaps too few, and some of them not taken with sufficient care, as in the first experiments I was not sufficiently acquainted with the working of the instrument to take the necessary precaution for freeing it of air, as in these trials it was generally placed above the water-level. In the Fiery Cross these objections are removed.

HEIGHTS OF MERCURY. SCALE $\frac{1}{8}$TH INCH = 1 INCH.



INSTRUMENT SHOWN $\frac{1}{8}$THS OF FULL SIZE.

	Values of n in the formula $v = n\sqrt{h}$	Knots observed. Velocity in knots per Hour.	Height of Mercury. Inches.
River Steamer VULCAN.....	4·61+	10·05	4·75
Do.	4·80+	14·32	8·9
Screw Steamer QUEEN.....		8·8	3·5
Do.		not observed.	4·5
Screw Steamer EMERALD.....	5·63	7·54	1.8
Do.	5·86	10·48	3·2
Screw Steamer LANCEFIELD.....			7·4
Screw Steamer FIERY CROSS.....	5·44	13·66	6·3

The curves on the accompanying sheet are drawn for different values of n from the formula $v = n\sqrt{h}$ to facilitate the formation of scales.

The working of the instrument on board the Fiery Cross, a screw steamer of about 1100 tons, on a recent voyage from the Clyde to Cardiff is shown in the following abstract from her log:—

FIERY CROSS—FROM THE CLYDE TO CARDIFF.

Time.	Revolutions of Screw per Minute.	Pressure on Boiler. lbs.	Height of Mercury in Speed Indicating Inches.	Velocity calculated by the formula $v = 5{\cdot}4\sqrt{h}$. Knots per Hour.
9.30 P.M.	52	...	5·2 } full steam.	12·3
11.50 „	47½	8¼	4·6 }	11·5
12.1 „	51½	12	5·15 expansively.	
12.25 „	54	15	5·5	
12.27 „	55	16	5·55	12·7
12.45 „	50		4·85 calm weather.	11·8
1.5 A.M.	52		5·15	
2 „	49		4·5	
9.55 „	49		4 2 ship rolling.	11
10 „	48¾		4·2 blowing fresh.	
2 P.M.	48½		3·95 with heavy seas.	10·7



PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

FIFTY-THIRD SESSION.

1st November, 1854.—WALTER CRUM, Esq., F.R.S., *President, in the Chair.*

DR. JOHN TAYLOR, Professor of Natural Philosophy in Anderson's Institution, was elected a member, having been proposed in the last session.

Mr. Liddell reported that the deputation appointed by the Council to proceed to the meeting of the British Association in Liverpool, in the month of September, and invite that body to hold its next Annual Meeting in Glasgow, had fulfilled its instructions, and that the Association had agreed to accept of the invitation, and resolved to hold its next meeting in Glasgow, in September 1855.

A letter from the Secretary to the Lords of Her Majesty's Treasury was read, dated July 11, 1853, acknowledging receipt of the Society's memorial on the subject of Patents.

The President read "Minutes of the Life and Character of Dr. Joseph Black," and afterwards communicated a Notice from Professor Liebig of a discovery in Cholera Prevention, which has been made in Munich by Professor Thiersch and Professor Pettenkofer. The latter paper called forth some remarks from Dr. Crawford as to the imperfect nature of the observations described.

November 15.—The Society met this evening. On taking the chair, the President communicated the melancholy intelligence of the death of Mr. Liddell, the Treasurer, which had occurred on the morning of that day; and concluded some remarks on the great personal worth and usefulness of Mr. Liddell, and on his services to this Society, by moving that it be remitted to the Secretaries to prepare a memorial of that gentle-

man, to be inserted in the Proceedings; and that as a mark of respect to the memory of its Treasurer, and an expression of sorrow for the loss it has sustained, the Society do now adjourn till the next Ordinary Meeting.

Mr. Hastie, in seconding the motion, followed up the remarks of the President in the same spirit; and the Society accordingly adjourned without proceeding to business.

DEATH OF ANDREW LIDDELL, ESQ.

Glasgow has lost one of its worthies by the death of Mr. Andrew Liddell. This mournful event took place at his residence, Bardowie House, on Wednesday morning the 15th of November. He became indisposed on Monday week, but his illness, which was a bilious fever, did not assume an alarming aspect till Sabbath. His strength continued gradually to sink till a few minutes after two o'clock on Wednesday morning, when he expired.

Mr. Liddell was born in 1786 in the village of Bainsford, near Falkirk, where his father was a schoolmaster. He received the elements of his education in that village and in Falkirk. When about thirteen years of age he went to assist his father at Carron iron-works, where he had been appointed a clerk. Here he remained till he was about eighteen years of age, when he removed to Edinburgh, and obtained a situation as clerk in a foundry. He next held a similar situation in the employment of Robert Anderson & Co., metal merchants, Leith. At this period, he commenced to study for the medical profession, and through the considerate kindness of his employers, to which he often reverted with pleasure, he was allowed to write up his books at night in order that he might have time for attending College during the day. He was diverted from his purpose of becoming a surgeon, after he had attended one or two of the classes in the University, by visiting Glasgow, where he was offered a partnership in an ironmongery establishment in Coach Court, Gallowgate. He closed with the offer, and settled in Glasgow about 1814 or 1815. In a few years all the partners retired, and his half-brother, Mr. Robert M'Laren, advanced capital, and went in as a sleeping partner, under the firm of Andrew Liddell & Co. In 1826, Mr. Liddell removed his place of business to 102 Argyle Street, his workshop being behind the front shop; but when the Arcade was projected in 1828, the workshop was removed to Washington Street, where the business of iron and brass founding was carried on, together with the manufacture of malleable iron pipes, the most extensive in Scotland. He had been making iron pipes in the usual way, when a new mode of welding was patented by Mr. Russell of Wednesbury in Staffordshire. Mr. Russell commenced a process against Mr. Liddell; but, by the interference of friends, the connection which began with a law-suit ended in

giving Mr. Liddell a right to the new process, and he carried it on until he retired from business. Next to the sturdy common sense, the practical sagacity, and energy of purpose which formed the prominent features of Mr. Liddell's mind, he was remarkable above most men for methodical habits and punctuality. These qualities, added to a good knowledge of machinery, rendered him a first-rate man of business; and the promptitude, energy, "push," and punctuality, which he exercised in his early career, characterized him through life. His success in business was equal to his wishes, and he was able to retire in 1844, with a respectable competency, and a large heart and liberal mind to enjoy the well-earned fruits of his toils, and employ a due share of his substance in doing good to others. Mr. M'Laren died in 1830, and Mr. Liddell continued sole partner in the business till 1844, when he surrendered the iron manufactory to his nephew, Mr. Robert M'Laren, Globe Foundry. Mr. Liddell was extensively employed, on the introduction of gas-light, as a fitter of gas-pipes and machinery. In addition to various towns in Scotland, he introduced gas into Armagh, Dungannon, Dundalk, and Kilkenny in Ireland; and sent apparatus for the same purpose to Nova Scotia and Canada.

Both before and after his retirement from business, Mr. Liddell took an active share in public affairs, for which his habits admirably qualified him. He served for several years in the Magistracy with great acceptance to the public. Amidst his busiest years, he never lost the thoughtful habits of his youth. He was a well read man in general literature; and was quite an authority on the subject of patented inventions, possessing in his valuable library one of the few complete copies of the "Repertory of Inventions," which he appeared to have thoroughly studied. He took a great interest in the British Association for the Advancement of Science, enjoyed the confidence of its leading men, and was a principal means of bringing that learned body to Glasgow in 1841, as well as of preparing, by a year of extraordinary personal labour in correspondence and otherwise, for its reception. He was one of the deputation who went from this city to the Liverpool meeting of 1854 to invite the Association to visit Glasgow next year, and had again promised his invaluable services in the way of rendering that visit interesting, useful, and creditable to our town. Some years ago, Mr. Liddell devoted much of his attention to reviving the public utility of Stirling's Library. His latest service of a literary kind was to write a biographical sketch of the celebrated David Dale, for the supplementary volume of Messrs. Blackie's "Lives of Eminent Scotsmen." Mr. Liddell was a man of a kindred mind with the philanthropic Dale, and undertook and completed the task *con amore*.

Mr. Liddell was one of the founders of the Night Asylum for the

Houseless, which will remain a permanent monument of his indomitable perseverance and practical benevolence. When the institution had commenced its operations in an old granary in St. Enoch's Wynd, he could scarcely obtain as many coadjutors as were required to give the experiment a trial. Nothing daunted, however, he resolved that the plan should succeed; and bated nought either of heart or hope, when, as he used to tell with glee, one of the public meetings about this period, consisted of himself in the chair, from which he proposed the resolutions, and his clerk or shopman, brought up for the occasion to second them. The success of the experiment, and its manifest benefits to homeless wanderers, arriving friendless in the town, and to poor people suddenly deprived of work and lodgings, and turned out into the streets, became speedily apparent. Mr. Liddell at length surrounded himself by a numerous and energetic body of fellow-labourers, whom he induced to embark in the building of the extensive and commodious house in North Frederick Street; and he was mainly instrumental in raising the funds for its erection. The additional feature of an industrial department was now added to the institution, for the protection and employment of females; and from the moral and economical advantages of this part of the system, Mr. Liddell derived peculiar satisfaction. This was his favourite topic at the annual meetings, when he delighted to expatiate upon individual instances which had come to his knowledge, of respectable females having been put in the way of maintaining themselves in comfort and independence by means of the encouragement afforded by this department of the institution.

As a Magistrate he dispensed justice with inflexible impartiality. He set his face sternly against vice in every form. He was literally a terror to evil doers. During his incumbency of office, he employed all his influence in diminishing the temptations to intemperance. In the young brought before him he felt a tender interest; and we have known him leave the Bench, where he had been investigating into a case of incipient juvenile delinquency, and proceed in quest of the parents of an erring boy to advise them as to his future conduct.

His early scientific education and practical knowledge of mechanics disposed him to take a warm and active interest in the Philosophical Society of Glasgow. He was admitted a member in 1819; was repeatedly elected President; and for many years held the office of Treasurer greatly to the benefit of the Society. Twenty years ago, when the Society had fallen to a low ebb, he and one or two other members preserved it from becoming extinct; and not unfrequently the meetings consisted only of himself and another, who, however, duly entered their sederunt in the minutes. It was principally through his influence that the Society was revived, and became, as it still continues, the centre and

rendezvous of the theoretical and practical science of the city and neighbourhood. It was in connection with this Society that Mr. Liddell took a leading part in organizing the great popular exhibition of the operations and products of the arts and sciences during the holidays at the close of 1846 and the beginning of 1847, to which nearly a hundred thousand visits were made, and which, after paying expenses, left a surplus which has fructified under his care into the sum of £619 for future use.

Mr. Liddell was brought up in the Scottish Baptist connexion; and when he retired from business in 1844, was invited to become the pastor of the church assembling in Brown Street, and of which he has been the sole pastor for the last three years. The chapel in Brown Street was originally built for an Independent congregation, but was purchased by Mr. Liddell for the use of the Baptist church. He officiated for the last time in his pastoral capacity on Sabbath the 5th of November. His attachment to the congregation led him, when residing at Plean, near Stirling, to come to Glasgow every Sabbath to join in its worship. Although conscientiously attached to his own denomination, he always cherished a most catholic feeling towards evangelical Christians of every name. His conversation on religious subjects was grave, earnest, and edifying. His Christian profession was humble, childlike, and self-questioning. His pious counsels to others were tendered with singular modesty and affection, and an utter absence of a dictatorial or overbearing spirit. His deeds of active benevolence, done in secret, but sometimes revealed by the objects of them, abounded in the neighbourhood of Plean and of Bardowie, in Glasgow, and wherever he went. Heart and hand were open to relieve distress and to do good. The wealth with which Providence had blessed his industry and skill in business, he held as a trust, to be administered as a faithful steward. Those who knew him most intimately will have observed, of late years, that his desire was to withdraw himself more and more from the engagements of active public life, and spend his declining years in the enjoyment of the elegant taste and warm affections of his happy home, and in the society of his attached friends, in a manner becoming the great change for which he was manifestly ripening, and to which he was often reaching forward in fond expectation. He has gone at last, to his own unspeakable gain, but to the heartfelt sorrow of his friends, who revered him for his sound judgment, his common sense, and practical sagacity,—who loved him for his genial kindness of heart, —who will miss his portly form, his radiant smile, his cordial greeting, and his ready pleasantry; and who will long cherish the memory of Mr. Andrew Liddell, as a fine example of a consistent Christian and a true man.

November 29, 1854.—WILLIAM GOURLIE, Esq., Vice-President, *in the Chair.*

DR. JOHN TAYLOR was admitted a member.

The following gentlemen were elected members, viz.:—Mr. Alexander Sinclair, Teacher, 25 St. George's Road; Mr. James Hunter, Iron-Master, Newmain's House, Motherwell; Mr. Charles O'Neil, Civil Engineer, 66 South Portland Street; Mr. Robert Calvert Clapham, Chemist, Ardeer Chemical Works, Stevenston, Ayrshire; Mr. James Murdoch, Pharmaceutical Chemist, 143 Sauchiehall Street.

The Society then proceeded to the election of its Office-bearers. It was agreed that the election to the vacant offices of President and Vice-President be by open vote.

Mr. Gourlie moved that, in accordance with the unanimous recommendation of the Council, Dr. Allen Thomson, Professor of Anatomy in the University of Glasgow, be appointed President.

The motion was seconded by Mr. William Murray, and carried by acclamation.

Dr. Thomson having taken the chair, and returned thanks for the honour conferred upon him, proposed that a resolution in reference to the retirement of Mr. Crum from office should now be entered upon the record.

This proposal was unanimously agreed to, and the following Resolutions expressing the sentiments of the Meeting is entered accordingly, and ordered to be communicated to Mr. Crum:—

"On Mr. Crum now vacating the President's Chair, which he does in consequence of the recent law of the Society which makes the tenure of that office biennial, the Philosophical Society feel themselves called upon to enter upon their record, and to convey to Mr. Crum, the expression of the deep obligation under which the Society has been laid by his long-continued and able exertions in forwarding its interests and welfare. They would also express more particularly their cordial approval of the zeal, judgment, and affability with which Mr. Crum has administered the affairs of the Society, not only during his recent short tenure of the office of President, but also for the longer previous period in which, through the infirm health of the late Dr. Thomas Thomson, a large share of the duties of the Chair devolved upon Mr. Crum as Vice-President.

"In conveying this resolution of thanks to Mr. Crum, the members of the Society would also desire to express the sincere regard and esteem which they entertain for his personal character, and the high respect in which they hold his scientific attainments, together with their earnest hope that Mr. Crum will continue to manifest the same interest as heretofore in the welfare of the Philosophical Society."

It was then moved by the President, that Mr. Gourlie and Mr. Alexander Harvey be appointed Vice-Presidents, which was agreed to by acclamation.

The President also moved that Mr. Cockey be appointed to the conjoint offices of Treasurer and Librarian; and that Mr. Hastie and Mr. Keddie be re-elected Secretaries; which motions were unanimously agreed to.

The Society then proceeded to elect by ballot the members of Council. Mr. Michael Connal and Mr. William M'Bride were requested to act as Scrutineers, who accordingly retired to examine the votes.

Mr. Cockey made a verbal report on the state of the Library, and announced the presentation to the Society, by Mr. Hastie, M.P., of a complete set of the Census Reports for 1851. Thanks voted.

The following abstract of the Treasurer's Account, Session 1853-54, was given in:—

1853.	Dr.						
Nov. 1.—To Cash in Union and Savings Banks,		£149	11	10			
— Interest on Banks' Accounts,		4	0	6			
					£153	12	4
1854.							
Nov. 1.—To Society's Transactions sold,					0	10	6
— Entries of 14 new Members, at 21s.		14	14	0			
— 11 Annual Payments from Original Members, at 5s.		2	15	0			
— 246 Annual Payments, at 15s.		184	10	0			
					201	19	0
— Rent from Sabbath School Teachers, for use of Hall,					2	10	0
					£358	11	10
1854.	Cr.						
Nov. 1.—By New Books and Binding,					£106	2	9
— Printing Transactions, Circulars, &c.					32	2	0
— New Bookcases, Upholstery, &c.					53	3	5
— Rent of Hall, 1 year,		£15	0	0			
— Fire Insurance,		4	19	0			
— Society's Officer and Clerk,		6	1	0			
— Postages and Delivering Letters,		7	14	0			
— Stationery,		1	19	6			
— Gas and Candles,		1	8	6			
					37	2	0
Carry forward,					£288	2	0

	Brought forward,			£288	2	0
Nov. 1.—By Librarian's Salary, 1 year,.....	£20	18	0			
— Do. for Poundage Collecting Dues,	7	6	0			
				37	4	0
— Subscription to Ray Society,.....	1	1	0			
— Do. to Palæontological Society,.....	1	1	0			
				2	2	0
— Cleaning the Hall and petty charges,.....				1	0	0
— Balance—						
Cash in Union Bank,.....	80	0	0			
Do. in Savings Bank, ...	9	15	8			
				89	15	8
				£358	11	10

THE PHILOSOPHICAL SOCIETY EXHIBITION FUND.

1853.				
May 15.—To Balance, as per deposit receipt, from the Corporation of the City of Glasgow, ...	£595	19	11	
1854.				
May 15. — Interest till this date,.....	23	16	9	
	£619	16	8	

GLASGOW, *25th October*, 1854.—We have examined the Treasurer's Account, and compared the same with the Vouchers, and find that there are in the Union Bank of Scotland Eighty Pounds, and in the Savings Bank Nine Pounds Fifteen Shillings and Eightpence —together, Eighty-nine Pounds Fifteen Shillings and Eightpence sterling—at the Society's credit at this date.

The Treasurer has also exhibited to us a Voucher which he holds for money lent to the Corporation of the City of Glasgow from the proceeds of the Philosophical Society's Exhibition in 1846, with Interest thereon to 15th May last, being Six Hundred and Nineteen Pounds Sixteen Shillings and Eightpence.

THOMAS DAWSON.
MATTHEW P. BELL.

REPORT BY THE TREASURER, 3D NOVEMBER, 1854.

The Furniture and other moveable property of the Society, with exception of printed Books, a new Bookcase, and Floor Cloth, remain the same as last year. The Funds in the Bank, as shown in the above Account, are £64 less than at the same period last year, caused chiefly by new Furniture got for the Hall. Reference is now made to the Librarian's Catalogue for the list of Books, the property of the Society as at this date.

The number of Members at commencement of Session 1852-3 was.....	263
New Members admitted during the Session,.....	14
Old Member replaced on Roll,.....	1
	278

From this 19 fall to be deducted from the Roll at commencement of Session 1854-5, viz.:—

Resigned Membership by letter,	5	
In arrear of Dues for two years, and held as resigned by Law XI.	4	
By request placed on Non-Resident List, having removed from Glasgow and paid Dues,	2	
In arrear of Dues for one year, but have left Glasgow for foreign countries, or places unknown,	4	
Dead,	4	
	—	19
On List for 1854-5,		259

In the above-named list of 259, there are 16 Members in arrear of Dues for one year, but as they all reside in Glasgow or neighbourhood it is hoped that these will be recovered.

Mr. John Thomson, C.E., described the process of Laying Down the Submarine Telegraph Cable in the Mediterranean.

The Scrutineers having given in their report, the following were declared to be the Office-bearers of the Society for the year 1854-55, viz.:—

President.

DR. ALLEN THOMSON.

Vice-Presidents.

MR. WILLIAM GOURLIE. | MR. ALEXANDER HARVEY.

Treasurer and Librarian.

MR. WILLIAM COCKEY.

Secretaries.

MR. ALEXANDER HASTIE. | MR. WILLIAM KEDDIE.

Council.

DR. THOMAS ANDERSON.	DR. A. K. YOUNG.
MR. WALTER CRUM.	MR. W. J. MACQUORN RANKINE.
MR. ROBERT BLACKIE.	MR. J. P. FRASER.
MR. JAMES NAPIER.	MR. WALTER NEILSON.
MR. JAMES BRYCE.	MR. JAMES COUPER.
MR. JOHN CONDIE.	MR. WILLIAM RAMSAY.

December 13, 1854.—*The* PRESIDENT *in the Chair.*

ON the reading of the Minutes of last meeting, Mr. Crum returned thanks for the kind sentiments expressed towards him in the Resolution placed on the Society's record, on the occasion of his retiring from the Chair.

The following gentlemen were elected members, viz., The Hon. Andrew Orr, Lord Provost of the City of Glasgow; Mr. William Euing, Insurance Broker; Dr. William Aitken, Demonstrator of Anatomy in the University of Glasgow; Mr. John H. Swan, Commission Agent; Mr. William Smith, Soap-maker; Mr. N. Mathieson, Soap-maker; Mr. Thomas King, Engineer; Mr. John Wardrope, Merchant; Mr. David Sutherland, Goldsmith; Mr. James L. Duncan, Surgeon-Dentist; Mr. Alexander Lister, Engineer; Mr. James Wilson, Merchant.

Donations to the Library of their Proceedings were received from, and thanks voted to, the Royal Scottish Society of Arts and the Literary and Philosophical Society of Liverpool.

Mr. Paul Cameron read a paper "On the Deviations of the Compass in Wooden and Iron Ships; illustrated by Models and Experiments bearing on Dr. Scoresby's paper at the British Association, and Professor Airey's reply."

January 10, 1855.—*The* PRESIDENT *in the Chair.*

THE following gentlemen were elected members, viz., Mr. Robert Williams, Government Inspector of Mines, 126 West Campbell Street; Mr. Robert Mansel, Ship Draughtsman, Govan; Mr. Walter M'Farlane, Iron Founder, Saracen Foundry; Mr. Alexander Whitelaw, Soap Manufacturer, 41 Sydney Street; Mr. Thomas B. Dalziel, Manufacturer, 99 Mitchell Street; Mr. James Reid, Banker, 23 Blythswood Square.

A letter was received from Messrs. M'Clure and Sons, presenting to the Society a framed copy of the engraved portrait of Professor Thomas Graham, after the original in the possession of the Society.

On the motion of Mr. Crum, seconded by Mr. Gourlie, the thanks of the Society were voted to Messrs. M'Clure for their gift.

A letter from Mr. W. J. Macquorn Rankine, addressed to the Secretaries, was read, recommending the adoption of measures for having the manufactures of Philosophical Instruments in Glasgow and elsewhere adequately represented in the approaching Universal Exhibition at Paris.

Mr. Rankine was heard in support of his recommendation, and moved the following resolution, which was seconded by Professor William Thomson, and approved of:—

"That a Committee be appointed, consisting of Members of the Society who are interested in or producers of Philosophical Instruments, with power to add to their number, in order to take into consideration the means of securing an adequate representation at the approaching Universal Exhibition in Paris of the manufacture of Philosophical Instruments in Glasgow, and in Scotland generally, and to communicate and co-operate with Committees, Societies, or other bodies or parties who may have a similar object in view.

"Mr. Walter Crum, of Thornliebank; Dr. Thomas Anderson; Prof. W. Thomson; Mr. J. R. Napier; Mr. W. Gale; Mr. Neil Robson; Mr. James Thomson, C.E., Belfast; Mr. W. Gardner; Mr. Paul Cameron; Mr. Alexander Mitchell; Mr. D. Mackain; Dr. John Macadam; Mr. Thomas R. Gardner; Mr. James King; Mr. Hughes; Mr. Robert Hart, Govan; Mr. John Finlay, 46 Buchanan Street; Mr. Malcolm M'Neill Walker, 24 Clyde Place; Dr. Strang; Professor Eadie; Mr. Simpson; the other Members of Council of the Society not named in the above list; Mr. W. J. Macquorn Rankine, Convener."

Mr. Hart described two luminous spots he had recently observed on the Moon's disc.

The President read a paper "On some Recent Discoveries with respect to the Impregnation of the Ovum in Fishes and other animals."

Mr. James R. Napier read "Remarks on Ships' Compasses," and was requested by the Society to recapitulate his paper at next meeting, in order to its being discussed on that occasion.

January 24, 1855.—*The* PRESIDENT *in the Chair.*

THE following gentlemen were elected members:—Mr. William Neilson, Insurance Agent, 69 Glassford Street; Mr. James King, Hurlet and Campsie Alum Co., Glasgow; Mr. Thomas Henderson, Merchant, 45 Union Street; Mr. James Greenshields, Chemist, 110 Peel Terrace.

Mr. Rankine reported the progress of the Committee on Philosophical Instruments for the Paris Industrial Exhibition.

Mr. Hart produced a letter from the Astronomer Royal, stating that he had no doubt that one of the spots observed on the moon by Mr. Hart was an occulted star. As to the other, it was in a region of the moon which the Astronomer Royal had often studied as an amateur, but he had never seen the phenomenon described.

Remarks on Ships' Compasses. By JAMES R. NAPIER.

It will be admitted by most persons, that all other things being equal, the best compass is that which has the strongest magnetism in its needles, or the most directive power in proportion to the whole weight of card and needles; for, by having this superior directive power, it is enabled to overcome pivot friction and other causes, which render useless less powerful compasses.

Compass makers evidently aim at developing in their needles as much magnetism as possible, and Dr. Scoresby's principle of making them hard in order to retain this development is, I believe, generally admitted and adopted. I conceived, therefore, that after the numerous experiments

published in his "Magnetical Investigations," there could be no doubt that a compass with a number of thin needles would be much more powerful than if the same weight of steel formed one solid bar needle. I was surprised, however, at finding it stated in Sir Snow Harris's "Rudimentary Treatise of Magnetism" (page 147), that no sufficient reason can be assigned for the employment of from three to five compound magnetic bars of costly and difficult construction, supposing it were proved from the evidence of experience as well as theoretically, that a single and simple edge-bar needle is even more than adequate to any required practical purpose. The object in using more than one needle (as noticed in page 141) is evidently a greater directive force, &c. This advantage, however, as Professor Barlow considers, "cannot be obtained without an increase of weight of steel, and, as a necessary consequence, a greater amount of friction on the pivot of suspension. Unless therefore the directive force increase in a greater ratio than the loss by friction and wear of the centre, little advantage is gained."

To satisfy myself I had a compass made, which I conceived would combine the good qualities of most of the compasses I had read about. I adopted Scoresby's compound needles made of clock springs, placed them for economy flat on the card in two sets, four in each set, as the objection to the use of one needle on the flat is I conceive much diminished where there are four, and still more so where there are eight needles. I placed these two sets of needles at the distances calculated by Mr. Archibald Smith, viz. 60°, shown by him to be the position (of the needles) in which the compass card might pitch or roll in all positions without affecting its direction. I tested the power of the card, with its needles complete on it, by the *torsion* balance, as I did not know how to apply Scoresby's deviation method where there were two sets of parallel needles. This method, when the *torsion* of wire is divided by the weight of card, gives I conceive a true measure of the powers which different cards would have of indicating correctly by overcoming friction, &c., when placed on their proper pivots.

The card with its needles was nearly of the same weight as one of Sir Snow Harris's, made by Lilley of London. The result of trials made in January, 1853, was as follows:—

Sir Snow Harris's single bar needle and card, } 840 grs. was deflected 90° by 29¼° torsion of wire.

And the Scoresby needle and card of } 838 grs. was deflected 90° by 48⅛° torsion of wire.

So that as the weights of the cards are about equal, their relative powers are about 29¼ to 48⅛, or 100 to 162. This compound needle card has therefore about 62 per cent. more power than Sir Snow Harris's single bar needle card. A recent trial with a different *torsion* wire of the same ex-

perimental card newly magnetized, compared with another of Sir Snow Harris's, also newly magnetized, gave the following results:—

Single bar needle,....932 grs. deflected 90° by 82° torsion of wire.

Scoresby's principle,..838 grs. deflected 90° by 121° torsion of wire.

And the comparative powers found after dividing the torsion by the weight are consequently 888 to 1453, or 100 : 164, so that the experimental Scoresby needle card has here about 64 per cent. more power than Sir Snow Harris's single bar needle. A comparison of other cards on the apparatus which Mr. Walker (a member of this Society) has kindly supplied me with, gives the following results:—

	Weight. Grains.	Power. *Torsion* Weight.
Experimental card, compound needle, ...	838	 1330
Captain Walker's card, T shaped needle, ...	989	 1203
Sir Snow Harris's azimuth card, edge bar needle,	820	 1090
Admiralty card, compound needles, ...	2023	 1000
Sir Snow Harris's steering card, edge bar, ...	932	 794
Gray's (Liverpool) card, two dipping needles,...	1708	 515
Keen's (Liver.) patent card, two edge bar needle,	2040	 442

Therefore, in regard to this particular card, at least sufficient reason can be given for the employment of even eight magnetic bars, viz., with cards of equal weight, the compound needle card has about one-half more power; or if, as Sir Snow Harris says, the extra power is not needed, then a much lighter card with less pivot friction, and consequently more durability, will be as powerful and sensitive as Sir Snow Harris's. If, indeed, the cost of the latter article, as supplied to the public, is any indication of the difficulty of its construction, it would appear that this particular arrangement of Scoresby's needles is much less difficult.

Sir Snow Harris, however, evidently here refers to the Admiralty compass card, which certainly does not appear of very easy construction. But, I am not aware that Dr. Scoresby in any of his works recommends this *particular* arrangement; and, when on a visit at Torquay about three years ago, I called on him, and he showed me his compasses and magnetic machines, and the needles of the former instruments were much more simply arranged.

The method he recommended to me was to bend the steel plates two or more on each side round the cap, and bring them nearly together, say within about $\frac{1}{4}$ of an inch, at the extremities or poles. I had a card or two fitted up on this plan for one of our steamers. The result of an experiment on it, however, was not so satisfactory as anticipated. I ascribed the failure to bad steel, or defective hardening, which Dr. Scoresby shows must have been the case, besides the fittings being too

heavy. Hence, the method already described has suggested itself as being simpler and more economical, and by experiment more powerful than any I had tried. I have preferred it. I therefore recommend this particular arrangement of needles for its simplicity, its economy, and its durability; for this same experimental card, after lying about for two years in every direction without any care being taken to have its poles protected, yet retains about twenty per cent. more power than one of Sir Snow Harris's recently magnetized. I presume from this example, therefore, that neither Mr. Cameron, who at the last meeting objected to compound needles on account of their tendency to lose their power, nor Captain Walker, who objects for the same reason, have literally followed Dr. Scoresby's instructions, or they would not have failed to make powerful and *enduring* compound needles.

So far from the compound needles of Dr. Scoresby losing their power, I have reason to know, from information received from that gentleman, that they are decidedly the most enduring of all others, of which he favours me with the following evidence:—

A compound needle of his construction, comprising six plates of thin steel, six inches in length, was put up in July, 1836, which was then found to have about double the power of any compass needle of the same weight which, at that time, he could meet with. Another of four plates, the same length, weighing *altogether* 579 grains, was put up August, 1839, which was considerably stronger than the other. These needles were placed in a case near to each other, mutually protecting, but were many times taken out for examination or experiment. After about sixteen years they were tested as to their residual powers, when the former was found to have lost only 6·6 per cent. and the other *less* than 6! Another needle of four plates was attached to a card, in February, 1839, and placed in a box *quite unprotected*, and exposed to various magnetic influences from magnets lying about. In May, 1841, the power (which had not been well recorded) was examined, and it was then singularly powerful. It was re-examined in the beginning of the present year, 1855, and was found, though entirely unprotected, to have lost only $7\frac{1}{2}$ per cent. in $13\frac{3}{4}$ years!

The Liverpool makers, Gray and Keen, seem to consider weight a necessary element of a *steady* card, if not of a powerful one, for their cards have a great weight of brass, paper, and talc, and very little magnetic steel in their construction. It will be allowed, however, that if weight alone, without much directive power of magnetic steel, is all that is necessary to make a steady card, a much less complex arrangement of parts could easily be contrived. Gray's card is about the same weight as the Admiralty standard card, and has only about half its power. In other words, Gray's card might be pointing steadily one or two points from the truth on account of its want of power to overcome the friction,

when the Admiralty, and all the more powerful cards, would be indicating correctly.

Many, if not all, compasses are said to be very unsteady at sea in heavy weather. The causes of these oscillations appear to me to be correctly described by Captain Walker in his "Magnetism of Ships," and also by Dr. Scoresby. One cause arises from the influence of the induced magnetism of the iron used in the construction of the ship, especially when the vessel sails in an easterly or westerly direction and rolls heavily, for then iron (the sides of an iron ship perhaps, or the iron davits of a wooden ship), which at one roll or lurch becomes nearly parallel with the dip, and consequently powerfully magnetic, attracts or repels the north end of the compass needle; by the opposite roll, may become nearly at right angles to the dip, and therefore less magnetic, thereby causing the oscillations.

The following experiments, made some years ago on a small iron boat about 8 feet long by $4\frac{1}{2}$ broad, will serve to illustrate these views:—

Boat's Head Magnetic.	Horizontal.	Inclined to Starboard.	To Port.
N.	0	+ $20\frac{1}{2}$	— 20
N.E.	+ $15\frac{1}{2}$	+ $24\frac{1}{2}$	+ 3
E.	+ $19\frac{1}{2}$	+ 15	+ 21
S.E.	+ $12\frac{1}{2}$	+ 5	+ 16
S.	+ 2	— 4	+ 7
S.W.	— 8	— $14\frac{1}{2}$	— 2
W.	— 15	— 18	— $12\frac{1}{2}$
N.W.	— 15	— $3\frac{1}{2}$	— $23\frac{1}{2}$
N.	— 0	+ $20\frac{1}{2}$	— 22

Many remedies have been suggested for preventing these oscillations. The most simple and effective, however, is that proposed and practised by Dr. Scoresby, so long ago as the year 1822, viz., to elevate the compass out of the reach of these troublesome attractions, and this method is now, though for a different object, frequently adopted.

Soft iron bars placed in the small iron boat horizontally on each or either side of the compass, reduced the errors when inclined to port and starboard, and it is presumable therefore would have diminished oscillations at sea from that cause; but, the practice is, I conceive, objectionable from the great care that I found necessary to preserve the iron from what is called retentive or retained magnetism, as slight knocks entirely altered the deviations.

The other cause of the oscillations, described by Captain Walker, is in the construction of the compass itself. The dip of the needle is counteracted generally, or always, in horizontal compass cards by a weight on the opposite end; and this weight by its inertia, Captain Walker says,

produces oscillations,—the more violent according to the power of the needles.

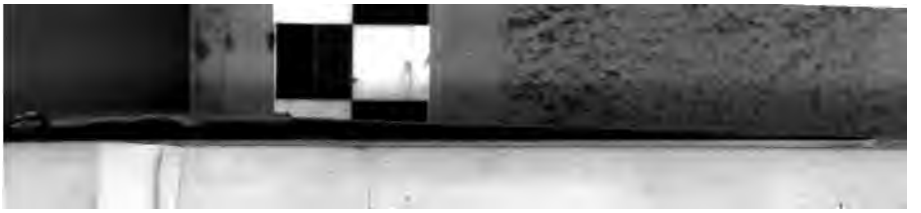
This I believe to be the case. The remedy for this appears to me to be very ingenious. The card traverses on an axis fixed to the top of a brass bell, which bell is placed on an ordinary pivot.

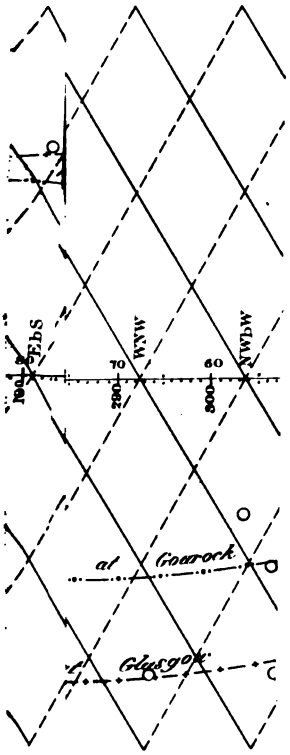
The weight which prevents the needle from dipping is here transferred to the bell, and the rotation or oscillation of the bell, he says, causes little or no oscillation of the needle itself, except what is communicated to it from friction against the axis and pivot. I do not know any simple method of testing the correctness of Captain Walker's views, but the reports of the Admiralty, and other trials of the compass published in Captain Walker's work, and also in the late Captain Johnstone's, prove it to be at least equal to the best.

While reading Dr. Scoresby's letter addressed to the Liverpool Underwriters' Association, on the subject of the Compasses of Iron Ships, I was startled by the remarks regarding the loss of the "Tayleur," when, from the evidence, he says it appeared that the compasses were all correct on leaving Liverpool, and all wrong after getting to sea. This recalled to my mind a singular change in the compass deviations of the iron brig "Haiti;" the change, however, was fortunately discovered before the vessel put to sea. She was built on the Clyde, near Glasgow, with her head about south-west, was swung as an experiment at the Broomielaw and the deviations noted, then was towed to Gourock Bay in order to be again swung and to have the errors reduced by soft iron correctors, but it blew a gale, which prevented the errors from being observed till the second day.

The deviations shown by the curve were then observed and noted, the results, as will be observed, differ considerably from those taken at Glasgow, (vide diagram, brig "Haiti.")

Though the observations at Gourock are evidently not so correct as could be wished, the curves passed through those that were thought to be the most so, show a great and decided difference, whatever may have been the cause of this change of the deviations, a compass placed up one of the masts, or on a mast, or staff of its own, would in all probability have avoided it; for, in the first place, there would have been little or no error in a compass placed at a proper height. Such changes, however, if arising, as appears to be very probable, if not certain, from the causes mentioned by Dr. Scoresby in his letter to the Liverpool Underwriters, viz., from the soft iron when hammered in a given direction, becoming highly penetrated with retentive magnetism, and so retaining its magnetism when the iron is turned quietly in any other direction, but being liable to change its character when vibrated in new positions, such change seems more likely to happen at Liverpool than on the Clyde, where vessels are fitted out in dock, and where the com-




E.bS
70
WNW
60
NWbW
Greenock
Glasgow



passes are adjusted. And as in sailing vessels an experimental trial seldom takes place, there is no opportunity for the magnetism retained while building or outfitting to be duly shaken or knocked into its natural position.

On the Clyde, on the contrary, our want of conveniences for swinging ships for compass deviations may have been the means of preserving our iron ships from those very sudden changes, as a voyage to Gareloch behind a tug steamer for two or three hours may have wrought the necessary change in the retained magnetism. With our steamers the effect of vibration could be more easily applied. And finding from Dr. Scoresby's experiments, that a mere slap of the hand is sometimes sufficient to change the polarity of a bar or plate of iron, I presume that the working of our engines for three hours or more on their way to Gareloch, and making more thumping and knocking perhaps than they ought to do, would be considered quite sufficient to redistribute the retained magnetism, so that when the ship was swung there would be less risk of any sudden change when first proceeding to sea.

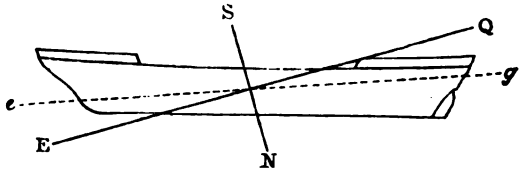
The following letter from Dr. Scoresby renders Mr. Cameron's statements about forging, hardening, and magnetizing steel bars in various directions doubtful, and suggests an interesting experiment which I shall now describe:—

"Torquay, *Jan.* 3d, 1855.

"My dear Sir,—I was interested by several of Mr. Cameron's statements in the letter he sent me, and, had I had time, I could have explained the reason (partially) why no change was *known* to take place in the compasses of certain ships which had been struck by the sea and returned to port. The change, if it occurred, would be upset on the ship's putting about and labouring by the sea with the head in a different direction. But Mr. C. altogether mistakes the effects of forging, hardening, and magnetizing, in the direction of the magnetic dip. I never found any (sensible) difference betwixt the magnetic force of a bar or needle, whether magnetized in the direction in which it had been hardened, or in the contrary. As to magnetizing, it is of no consequence, the direction, if the magnet used be sufficiently strong; for a much greater power is communicated than the steel can afterwards retain.

"Pray, is the ship you have to launch shortly with her keel *north* and *south*, magnetic, or *nearly so*? my impression is that it will probably be so. If so, an interesting and easy experiment could be made, viz., to ascertain where (or at what height) on the stem and stern the ship ceases to act on the compass? Or where, on either side of the bow and stern (say 6 to 10 feet from either, towards the middle of the ship) the line of attraction runs?

"*Theoretically*, in one point of view, all the head would have S polarity, and all the stern N. But my impression is, that in such a case, the equatorial line will be modified and brought *within* the vessel, so that the upper works, both forward and aft, might possibly have S polarity, and the keel forward and aft, *north*. Pray do try this.



"But with such a modification I should expect that the line of no-attraction would run obliquely through the ship, something like the dotted line *e g*, and so as, if not running out, to be highest *aft*, and lowest *forward*? The line of no attraction on the two sides (keel N or S magnetic) should be the same.

"I have just heard of another case of compass change by a shock, in the case of a collision, where a change of about 2½ points took place in the compasses!

"Yours very faithfully,

"W. SCORESBY."

Description of the Curves of no Deviation on the S.S. "Fiery Cross:"—

Fig. 1 represents the sheer plan of the S.S. "Fiery Cross" lying at the inclination in which she was built, and with her head pointing south 32° west.

The curved lines drawn from the fore foot to the quarter, denote the heights of the curves of no deviation, on either side, as indicated by a small 2½ inch compass needle, held with its centre 2 feet from the vessel's side. The small circles denote points of actual observation, and are those from which the curves are laid down. Thwartship, sections of the vessel are also shown at the points A, B, C, and D, and the positions of the centre of the needle, in respect to them, denoted by a small cross.

The small needle being moved upwards or downwards from the above indicated positions, experiences the rapidly varying resultant action of the induced magnetism of the iron of the vessel, in virtue of which arise large horizontal deviations in its direction. In the case of the needle being carried round the inside of the bulwarks, at a height of 2 feet above the iron gunwale, the magnitude and character of these deviations are exhibited by the curves drawn in figure 2. The line in the sheer plan, denoting the height at which the small compass was carried round the gunwale, has been repeated higher up, so as to form the axis for the curves, upon which axis the observed deviations have

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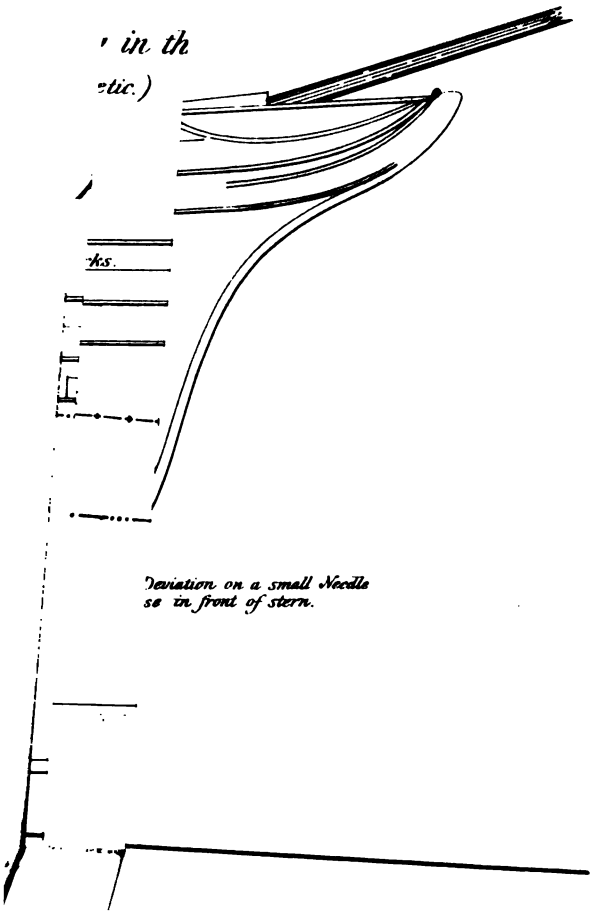
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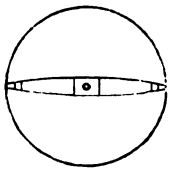
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Deviation on a small Needle
se in front of stern.



been laid off to a scale of 1 inch, equal to 120°. As in the former case, two sets of small circles denote actual observations, and are placed exactly over those places in the sheer plan at which the observations were taken in the vessel; further, where the curves lie above this axis the south end of the needle was attracted towards the side of the vessel, when under the north, was the attracted end; a second set of dotted circles represent a second set of observations made on the vessel after her launch, and when lying at Lancefield Quay, with her head pointing south-east, show a great change in the deviations.



While on this subject, I may mention a method suggested to me some time ago by Mr. Archibald Smith for finding the deviation of a steamer's compass. Its simplicity and the speed with which it can be executed are favourable to its adoption, and there can, I believe, be little or no oblique retained magnetism to interfere with the results, as the shaking of the vessel from the motion of the machinery must tend to bring it to its natural position.

The steamer is supposed to be in sight of a prominent object as far off as possible, and the bearings of this object taken; while the vessel steams steadily on each of the 32, 16, 8, or even 4 points of the compass by which the bearings are taken. The mean of these bearings measured all in one direction will give very nearly the true magnetic bearing of the distant object, and of course the difference of each bearing from the mean, is the error or deviation of the compass, which has to be properly allowed for.

As an example, I have taken the bearings from the deviation curve of the iron brig "Haiti" (vide diagram), for 4 and for 8 points.

Ship's Head by Standard Compass.		
N.	.....	+ 15
N.E.	.....	— 15
E.	.....	— 28
S.E.	.....	— 24
S.	.....	— 10
S.W.	.....	+ 10
W.	.....	+ 31
N.W.	.....	+ 36

Let the bearings of the distant object be N. when the ship's head by her standard compass is N.W. The other bearings will be as follows:—

Ship's Head.		Bearings of Dist. Object.
N.W.	.....	N.
N.	.....	N. 21° W.
N.E.	.....	N. 51 W.
E.	.....	N. 64 W.
S.E.	.....	N. 60 W.
S.	.....	N. 46 W.
S.W.	.....	N. 26 W.
W.	.....	N. 5 W.

8) 273

{N. 34·125 W.

Correct magnetic, ...N. 36 W.

Showing a difference of only $1°\frac{8}{10}$ from the correct bearing. When the bearings are taken on only four pivots these are best to be the diagonal points.

N.W.	.....	N. 0 W.
N.E.	.....	N. 51 W.
S.E.	.....	N. 60 W.
S.W.	.....	N. 26 W.
		4) 137
		N. 34·25 W.

Showing a difference of only $1°\frac{3}{4}$ from the truth.

Care must be taken that the deviations thus found be correctly allowed for, as Sir Snow Harris and the late Captain Johnstone have both erred in their directions for steering given courses.

The most simple and at the same time correct method of showing and applying the deviations, is the graphic method, suggested by Mr. Archibald Smith, and described in his "Supplement to the Practical Rules for Ascertaining the Deviations of the Compass, &c.," and also in the late Captain Johnstone's work on Compasses. (Vide diagram.)

I am not prepared to enter upon the subject of correcting compasses by magnets, as recommended by Professor Airy. Trustworthy observations have shown that in some instances compasses so adjusted have been correct in all the latitudes into which the vessel has sailed, while other observations have shown that the magnetic corrections have altogether failed. The accompanying remarks, with which Mr. Smith has kindly furnished me, puts the subject in a clearer and more satisfactory light:—

"LINCOLN'S INN, *January* 22, 1855.

"MY DEAR NAPIER,—The results I have got are these:—In wooden ships the magnetism is almost entirely that of soft iron, which only becomes magnetic by induction, but on a wooden ship changing her latitude it requires some weeks for the iron to get into its new magnetic condition. In such ships the principal part of the deviation varies as the *tangent* of the dip, and therefore becomes nil at the magnetic equator, and *changes* its sign on a change of hemisphere.

"In iron ships the greater part of the deviation seems to arise from permanent magnetism, and the deviation in the south to be in the same direction generally as in the north; but then the ships whose deviations I have examined had not been long in the south, and it may be that in time, and with blows or strains, the magnetism would have changed.

"The following considerations seem to show, conclusively, that no confidence can be placed in any prediction as to the changes an iron ship will undergo on a change of latitude.

"Suppose all the iron to be as to part perfectly hard, as to the rest perfectly soft. The true result must be between the extremes; and it is quite certain, that even in an iron vessel, there is a great deal of iron approaching to soft iron. We may have several cases.

"1. The compass may happen when placed where the deviation caused by the soft iron *compensates itself*, and where, therefore, the whole deviation is caused by the permanently magnetic iron. In this case, Airy will appear to be right, and the correction by magnets will answer. Curiously enough, I find that this was very nearly the case with the 'Trident,' the iron vessel which he selects as a test of his theory.

"2. The compass may be placed where the permanently magnetic iron compensates itself, and then the deviation would appear to be that of soft iron, and would change its sign on a change of latitude, and if corrected by magnets the error in the south hemisphere would be doubled. I have not found an instance of this.

"3. The compass may be placed where the deviation is small, from the hard and soft iron compensating each other. In this case, when the vessel goes to the south, the two magnetisms will act in the same direction, and there will be a large deviation. I think this is the explanation of the deviation of the 'Bolivia' which you sent me.

"4. The two deviations may, in England, act in the same direction, that from hard iron being the greatest. In this case, in a southern latitude, the Cape for instance, the deviation will have the same direction, but be diminished in amount. This was the case with one iron steamer, whose deviations I got from the Admiralty, I think the 'Birkenhead.'

"5. The two deviations in England may act in opposite directions, that from hard iron being the greatest. In that case in the south, the deviation will remain the same in direction, but will be increased. This was the case with another iron steamer, whose deviations I got from the Admiralty, I think the 'Vulcan.'

"Other causes may easily be imagined, and all show how uncertain any corrections by magnets, or even by soft iron pillars, which I was once inclined to, must be; and that the best thing to do is to keep the compass as far as possible from all iron, and make frequent observations, &c.

"ARCHIBALD SMITH."

Though many captains of vessels still adhere to the principle of having their compass errors corrected by magnets, the custom of placing a compass on a mast or pole high above the vessel to be free from the influence of iron, as urged by Dr. Scoresby and implied by Mr. Smith, is becoming more frequent. It is to be hoped this custom will soon become universal.

Note on the Determination of the Magnetic Meridian at a Distance from Land. By W. J. Macquorn Rankine, C.E., F.R.SS. L. & E.

The principle of the following method of approximately finding the magnetic meridian on board ship when at a distance from land, and some of the results of that principle, were communicated to the Royal Society in 1853, and an abstract of them published in the Proceedings of that body. In the present note the theoretical investigation is somewhat simplified, and the resulting formula is put into a shape more convenient for practical use; and a method is also explained of substituting a geometrical construction for some of the calculations.

The *principle* in question is this:—*When a ship performs a complete rotation, her head returning exactly to the point from which it started, the sum of the mechanical work performed by the horizontal rotative forces acting between the compass-needle and the earth, between the earth and the ship, and between the needle and the ship respectively, is equal to nothing.*

Let the following symbols denote angles, measured in the direction of motion of the hands of a watch, *i.e.*, from north to east:—

δ, the angle from the north end of the needle to the magnetic north = the *westerly* deviation of the needle, when positive.

ζ, from the magnetic north to the ship's head = true magnetic bearing of the ship's head.

$\zeta' = \zeta + \delta$, from the north end of the needle to the ship's head = apparent magnetic bearing of the ship's head.

Let the horizontal couples, or rotative moments, tending to vary the above angles, be denoted as follows:—

P, the couple acting between the needle and the earth ($= m X \sin \delta$, where m is the magnetic moment of the needle, and X the earth's horizontal force);

Z, the couple acting between the earth and the ship;

— P, that acting between the needle and the ship (being equal and opposite to that acting between the needle and the earth).

Then the principle stated above is thus expressed:—

$$-\int P\,d\delta - \int_0^{2\pi} Z\,d\zeta + \int_0^{2\pi} P\,d\zeta' = 0 \quad \ldots\ldots\ldots\ldots (1.)$$

Now the first term of this equation, $\int P\,d\delta$, being obviously null, it is reduced to

$$\int_0^{2\pi} Z\,d\zeta = \int_0^{2\pi} P\,d\zeta' = m X \int_0^{2\pi} \sin\delta \cdot d\zeta'$$

or, dividing both sides by $2\pi\, m X$,

$$\frac{1}{2\pi\, m X} \int_0^{2\pi} Z\,d\zeta = \frac{1}{2\pi} \int_0^{2\pi} \sin.\,\delta \cdot d\zeta' = A \quad \ldots\ldots\ldots\ldots (2.)$$

A denoting, as in Mr. Archibald Smith's notation, the *mean of the sines*

of all the deviations of the compass-needle (positive when westerly) observed in swinging the ship.

Now, it is probable that the *mean value* of Z, the rotative force acting between the earth and the ship, varies simply in proportion to X, the earth's horizontal force; and, consequently, that A is a constant for a given ship, for all positions on the earth's surface, which may be ascertained once for all, when the ship is in port.

The symbol $\frac{1}{2\pi}\int_0^{2\pi} \sin \delta \, d\zeta'$ denotes the taking of the mean of the sines of the deviations of the needle with the ship's head on an infinite number of equidistant apparent bearings. In practice, only a finite number of such deviations can be observed. Let the mean of their sines be denoted by

$$\mathfrak{m} \sin. \delta = A \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3.)$$

This quantity having been ascertained, once for all, let the ship be supposed to be at a distance from land, and let it be required to find the magnetic meridian. Let the ship be swung round, and let the apparent magnetic bearings of any fixed distant object be taken, with the ship's head on each of the thirty-two points, or on sixteen equidistant points, as the case may be. A star will answer for an object, if its apparent motion be allowed for by calculation. It is required to find the true magnetic bearing of this object.

Let α denote this true magnetic bearing, α' any one of the apparent magnetic bearings, then the corresponding deviation of the needle is

$$\delta = \alpha' - \alpha,$$

and consequently

$$\sin. \delta = \cos. \alpha \sin. \alpha' - \sin. \alpha \,.\, \cos. \alpha'$$

Therefore, let S denote the *mean of the sines* of the apparent bearings of the distant object, and C the mean of their cosines; then

$$A = S \cdot \cos. \alpha - C \cdot \sin. \alpha \ldots\ldots\ldots\ldots (4.)$$

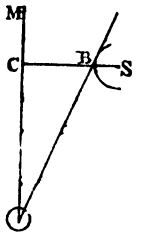
the solution of which equation gives, for the *true magnetic bearing of the object*,

$$\alpha = \text{arc. tan.} \frac{S}{C} - \text{arc. sin.} \frac{A}{\sqrt{C^2+S^2}} \ldots\ldots\ldots\ldots (5.)$$

The true magnetic bearing of a visible object having been thus determined, the magnetic meridian is known, and also the deviations of the needle for all those positions of the ship's head at which apparent bearings of the object were taken.

It is scarcely necessary to add, that in taking the means of the sines and cosines, sines of easterly bearings and cosines of northerly bearings are to be considered as positive; and sines of westerly bearings and cosines of southerly bearings as negative.

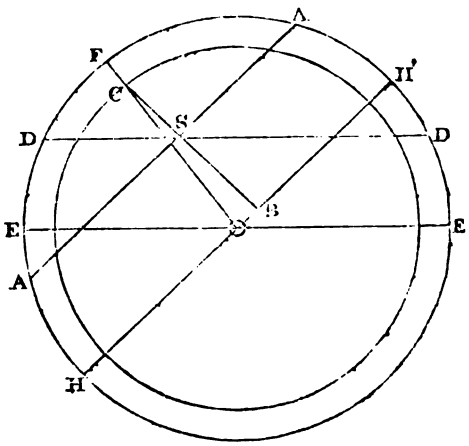
The following is the geometrical construction corresponding to the equation 5. Fig. 1 :—



Draw a straight line, O M, to represent the magnetic meridian, in which take any point, O. From O, set off on the line O M, O C = C, the mean of the cosines of the apparent bearings of the object. At C erect C S perpendicular to O M, and make C S = S, the mean of the sines of the apparent bearings. Round the point S, with a radius = A, describe an arc of a circle, to the left of S if A is a positive quantity, to the right if A is a negative quantity. From O draw O B touching that arc. Then is the angle M O B the true magnetic bearing of the object.

Note on the Approximate Determination of the Azimuth of a Star by Geometrical Construction, its Declination and Altitude, and the Latitude of the Place of Observation being given. By W. J. Macquorn Rankine.

To solve this problem geometrically, it is necessary to have a graduated circle drawn on a large flat piece of card-board. The drawing instruments required are a large pair of compasses, and a long and accurate straight edged ruler. The larger the circle, and the more minute the graduations, the more accurate will be the result.



Let E H' E H be the graduated circle, whose centre is at O. This circle

is to be conceived to represent an orthographic projection of the celestial sphere on the plane of the meridian. Let the straight line, E E, be the projection of the equator.

Set off the arcs E H = E H′ = the co-latitude of the place of observation; then will H O H′ be the projection of the horizon of that place. From the equator set off the two arcs E D = the declination of the star, and draw the straight line, D D, which will be the projection of the parallel of declination. From the horizon set off the arcs H A = H′ A = the altitude of the star, and draw the straight line A A, which will be the projection of the parallel of altitude. Then S, where A A intersects D D, will be the orthographic projection of the star.

Round O, with the radius O C = ½ A A, describe a circle, or part of a circle. From S let fall S B perpendicular to the horizon H O H′, and produce this perpendicular till it cuts the last mentioned circle. Let C be the point of intersection. Draw the straight line O C, and produce it till it cuts the graduated circle in F. Then will the arc H′ F be the azimuth of the star, measured from that pole which is above the horizon.

This method answers best when the star observed is at a distance from the meridian without, being so near the horizon as to be much affected by refraction, or so near the zenith as to make its azimuth uncertain.

A geometrical construction analogous to this has been used by the author to determine the apparent solar time from an observation of the sun's altitude.

Professor Gordon gave "An Account of New Formulas for Calculating the Strength of Pillars of Cast and Wrought Iron."

Mr. Ure exhibited a Lamp of a new construction.

February 7, 1855.—*The* PRESIDENT *in the Chair.*

THE following were elected members:—Mr. James Ferguson, Mining Engineer, Gas-Coal Works, Lesmahagow; Mr. James M'Intosh, Tanner, 129 Stockwell Street; Mr. Daniel Macnee, Painter, 132 West Regent Street; Mr. Wm. Robertson, C. and M.E., 97 Union Street.

On the recommendation of the Council, the Society agreed to grant a sum not exceeding £6 for the purchase of a new Black Board. The first vote was taken, and the motion was carried unanimously.

Mr. Bryce read "Notices of the Natural and Civil History of the Crimea—its Geology and Climate."

February 21, 1855.—*The* PRESIDENT *in the Chair.*

MR. JOHN BAWDEN, Engineer, was elected a member.

The Society, by its second vote, finally agreed to grant a sum of £6 for Black Boards.

Mr. James Elliott, Teacher of Mathematics, Edinburgh, at the request of the Council, read a paper "On certain Mechanical Illustrations of the Motions of the Planets, accompanied by theoretical investigations relating to these, and, in particular, a new Explanation of the Stability of Equilibrium of Saturn's Rings."

The reading of the paper was followed by a discussion, in which Professor Nichol, Professor William Thomson, and Dr. Taylor took part.

March 7, 1855.—*The* PRESIDENT *in the Chair.*

PROFESSOR NICHOL read a paper, entitled, "Saturn's Rings—a chapter of Scientific History."

March 20, 1855.—*The* PRESIDENT *in the Chair.*

MR. BRYCE proposed that a Committee should be appointed to collect observations illustrative of the extreme severity of the late winter.

The suggestion was also recommended by the President, and approved of by the Society.

The following Committee was accordingly appointed, viz., Mr. King, Windsor Terrace; Mr. Hart, Cesnock Park; Mr. Thomas Gardner, Buchanan Street; Dr. Anderson, College; Mr. Bryce, Convener.

Dr. Strang read Statistical Memoranda connected with the recent Social Progress of Paris.

Mr. Bryce gave an account of the general Geology and Glacial Phenomena of the Lake District of Cumberland and Westmoreland.

April 4, 1855.—*The* PRESIDENT *in the Chair.*

MR. JAMES B. MURDOCH was elected a member.

The President intimated that arrangements had been made by the Council for the delivery of a lecture on the Attack and Defence of Fortified Places, by Captain Maclagan of the Bengal Engineers, in the Merchants' Hall, next Wednesday evening; that invitations would be

sent to the Magistrates and other municipal authorities; and that the members of the Society would be furnished with tickets for their friends.

Dr. Taylor, Professor of Natural Philosophy, Anderson's University, read a paper "On the Nature and Causes of Hurricanes."

April 18, 1855.—*The* President *in the Chair.*

Professor William Thomson gave an account of "Recent Experimental Investigations in Thermo-Electricity."

May 2, 1855 (the Concluding Meeting of the Session was held this evening).—William Gourlie, Esq., Vice-President, *in the Chair.*

Mr. J. Napier read a paper "On the Chemistry of Trap Dykes in Arran."

Mr. W. J. Macquorn Rankine read a paper "On the Science of Energetics."

Outlines of the Science of Energetics. By William John Macquorn Rankine, Civil Engineer, F.R.SS. London and Edinburgh, &c.

I. What constitutes a Physical Theory.

An essential distinction exists between two stages in the process of advancing our knowledge of the laws of physical phenomena; the first stage consists in observing the relations of phenomena, whether of such as occur in the ordinary course of nature, or of such as are artificially produced in experimental investigations, and in expressing the relations so observed by propositions called formal laws. The second stage consists in reducing the formal laws of an entire class of phenomena to the form of a science; that is to say, in discovering the most simple system of principles, from which all the formal laws of the class of phenomena can be deduced as consequences.

Such a system of principles, with its consequences methodically deduced, constitutes the physical theory of a class of phenomena.

A physical theory, like an abstract science, consists of definitions and axioms as first principles, and of propositions, their consequences; but with these differences:—first, That in an abstract science, a definition assigns a name to a class of notions derived originally from observation, but not necessarily corresponding to any existing objects of real pheno-

mena, and an axiom states a mutual relation amongst such notions, or the names denoting them; while in a physical science, a definition states properties common to a class of existing objects, or real phenomena, and a physical axiom states a general law as to the relations of phenomena; and, secondly,—That in an abstract science, the propositions first discovered are the most simple; whilst in a physical theory, the propositions first discovered are in general numerous and complex, being formal laws, the immediate results of observation and experiment, from which the definitions and axioms are subsequently arrived at by a process of reasoning differing from that whereby one proposition is deduced from another in an abstract science, partly in being more complex and difficult, and partly in being to a certain extent *tentative*, that is to say, involving the trial of conjectural principles, and their acceptance or rejection according as their consequences are found to agree or disagree with the formal laws deduced immediately from observation and experiment.

II. The Abstractive Method of forming a Physical Theory, distinguished from the Hypothetical Method.

Two methods of framing a physical theory may be distinguished, characterized chiefly by the manner in which classes of phenomena are defined. They may be termed respectively the ABSTRACTIVE and the HYPOTHETICAL methods.

According to the ABSTRACTIVE method, a class of objects or phenomena is defined by describing, or otherwise making to be understood, and assigning a name or symbol to, that assemblage of properties which is common to all the objects or phenomena composing the class, as perceived by the senses, without introducing anything hypothetical.

According to the HYPOTHETICAL method, a class of objects or phenomena is defined according to a conjectural conception of their nature, as being constituted in a manner not apparent to the senses, by a modification of some other class of objects or phenomena whose laws are already known. Should the consequences of such a hypothetical definition be found to be in accordance with the results of observation and experiment, it serves as the means of deducing the laws of one class of objects or phenomena from those of another.

The conjectural conceptions involved in the hypothetical method may be distinguished into two classes, according as they are adopted as a probable representation of a state of things which may really exist, though imperceptible to the senses, or merely as a convenient means of expressing the laws of phenomena; two kinds of hypotheses, of which the former may be called *objective*, and the latter *subjective*. As examples of objec-

tive hypotheses may be taken, that of vibrations or oscillations in the theory of light, and that of atoms in chemistry; as an example of a subjective hypothesis, that of magnetic fluids.

III. The Science of Mechanics considered as an illustration of the Abstractive Method.

The principles of the science of mechanics, the only example yet existing of a complete physical theory, are altogether formed from the data of experience by the abstractive method. The class of *objects* to which the science of mechanics relates,—viz.,—material bodies,—are defined by means of those sensible properties which they all possess, viz., the property of occupying space, and that of resisting change of motion. The two classes of *phenomena* to which the science of mechanics relates are distinguished by two words, *motion* and *force; motion* being a word denoting that which is common to the fall of heavy bodies, the flow of streams, the tides, the winds, the vibrations of sonorous bodies, the revolutions of the stars, and generally to all phenomena involving change of the portions of space occupied by bodies; and *force*, a word denoting that which is common to the mutual attractions and repulsions of bodies, distant or near, and of the parts of bodies, the mutual pressure or stress of bodies in contact, and of the parts of bodies, the muscular exertions of animals, and, generally, to all phenomena tending to produce or to prevent motion.

The laws of the composition and resolution of motions, and of the composition and resolution of forces, are expressed by propositions which are the consequences of the definitions of motion and force respectively. The laws of the relations between motion and force are the consequences of certain axioms, being the most simple and general expressions for all that has been ascertained by experience respecting those relations.

IV. Mechanical Hypotheses in various Branches of Physics.

The fact that the theory of motions and motive forces is the only complete physical theory, has naturally led to the adoption of *mechanical hypotheses* in the theories of other branches of physics; that is to say, hypothetical definitions, in which classes of phenomena are defined conjecturally as being constituted by some kind of motion or motive force not obvious to the senses (called *molecular* motion or force) as when light and radiant heat as defined as consisting in molecular vibrations, thermometric heat in molecular vortices, and the rigidity of solids in molecular attractions and repulsions.

The hypothetical motions and forces are sometimes ascribed to *hypo-*

thetical bodies, such as the luminiferous æther; sometimes to *hypothetical parts*, whereof tangible bodies are conjecturally defined to consist, such as atoms, atomic nuclei with elastic atmospheres, and the like.

A mechanical hypothesis is held to have fulfilled its object, when, by applying the known axioms of mechanics to the hypothetical motions and forces, results are obtained agreeing with the observed laws of the classes of phenomena under consideration, and when, by the aid of such a hypothesis, phenomena previously unobserved are predicted, and laws anticipated, it attains a high degree of probability.

A mechanical hypothesis is the better, the more extensive the range of phenomena whose laws it serves to deduce from the axioms of mechanics; and the perfection of such a hypothesis would be, if it could, by means of one connected system of suppositions, be made to form a basis for all branches of molecular physics.

V. Advantages and Disadvantages of Hypothetical Theories.

It is well known that certain hypothetical theories, such as the wave theory of light, have proved extremely useful, by reducing the laws of a various and complicated class of phenomena to a few simple principles, and by anticipating laws afterwards verified by observation.

Such are the results to be expected from well-framed hypotheses in every branch of physics, when used with judgment, and especially with that caution which arises from the consideration, that even those hypotheses whose consequences are most fully confirmed by experiment, never can by any amount of evidence attain that degree of certainty which belongs to observed facts.

Of mechanical hypotheses in particular, it is to be observed, that their tendency is to combine all branches of physics into one system, by making the axioms of mechanics the first principles of the laws of all phenomena; an object for the attainment of which an earnest wish was expressed by Newton.*

In the mechanical theories of elasticity, light, heat, and electricity, considerable progress has been made towards that end.

The neglect of the caution already referred to, however, has caused some hypotheses to assume, in the minds of the public generally, as well as in those of many scientific men, that authority which belongs to facts alone, and a tendency has consequently often evinced itself to explain away, or set aside, facts inconsistent with these hypotheses, which facts, rightly appreciated, would have formed the basis of true theories; thus

* Utinam cætera naturæ phænomena ex principiis mechanicis eodem argumentandi genere derivare liceret.—(*Phil. Nat. Prin. Math.*; *Præf.*)

the fact of the production of heat by friction, the basis of the true theory of heat, was long neglected, because inconsistent with the hypothesis of caloric; and the fact of the production of cold by electric currents, at certain metallic junctions, the key (as Professor William Thomson recently showed) to the true theory of the phenomena of thermo-electricity, was, from inconsistency with prevalent assumptions respecting the so-called "electric fluid," by some regarded as a thing to be explained away, and by others as a delusion.

Such are the evils which arise from the misuse of hypothesis.

VI. Advantages of an Extension of the Abstractive Method of Framing Theories.

Besides the perfecting of Mechanical Hypotheses, another and an entirely distinct method presents itself for combining the physical sciences into one system; and that it is by an *extension of the* Abstractive Process in framing Theories.

The abstractive method has already been partially applied, and with success, to special branches of molecular physics, such as heat, electricity, and magnetism. We are now to consider in what manner it is to be applied to physics generally, considered as one science.

Instead of supposing the various classes of physical phenomena to be constituted in an occult way of modifications of motion and force, let us distinguish the properties which those classes possess in common with each other, and so define more extensive classes denoted by suitable terms. For axioms, to express the laws of those more extensive classes of phenomena, let us frame propositions comprehending as particular cases, the laws of the particular classes of phenomena comprehended under the more extensive classes. So shall we arrive at a body of principles, applicable to physical phenomena in general, and which being framed by induction from facts alone, will be free from the uncertainty which must always attach even to those mechanical hypotheses whose consequences are most fully confirmed by experiment.

This extension of the abstractive process is not proposed in order to supersede the hypothetical method of theorizing; for in almost every branch of molecular physics it may be held, that a hypothetical theory is necessary as a preliminary step to reduce the expression of the phenomena to simplicity and order, before it is possible to make any progress in framing an abstractive theory.

VII. Nature of the Science of Energetics.

Energy, or the capacity to effect changes, is the common characteristic of the various states of matter to which the several branches of

physics relate; if, then, there be general laws respecting energy, such laws must be applicable, *mutatis mutandis*, to every branch of physics, and must express a body of principles as to physical phenomena in general.

In a paper read to the Philosophical Society of Glasgow on the 5th of January 1853, a first attempt was made to investigate such principles, by defining *actual energy* and *potential energy*, and by demonstrating a general law of the mutual transformations of those kinds of energy, of which one particular case is a previously known law of the mechanical action of heat in elastic bodies, and another, a subsequently demonstrated law which forms the basis of Professor William Thomson's Theory of thermo-electricity.

The object of the present paper is, to present in a more systematic form, both these and some other principles, forming part of a science whose subjects are, material bodies and physical phenomena in general, and which it is proposed to call the Science of Energetics.

VIII. Definitions of certain Terms.

The peculiar terms which will be used in treating of the Science of Energetics are purely abstract; that is to say, they are not the names of any particular object, nor of any particular phenomena, nor of any particular notions of the mind, but are names of very comprehensive *classes* of objects and phenomena. About such classes it is impossible to think or to reason, except by the aid of examples or of symbols. General terms are symbols employed for this purpose.

Substance.

The term "*substance*" will be applied to all bodies, parts of bodies, and systems of bodies. The parts of a substance may be spoken of as distinct substances, and a system of substances related to each other may be spoken of as one complex substance. Strictly speaking, the term should be "*material substance*," but it is easily borne in mind, that in this essay none but material substances are referred to.

Property.

The term "*property*" will be restricted to *invariable* properties; whether such as always belong to all material substances, or such as constitute the invariable distinctions between one kind of substance and another.

Mass.

Mass means "*quantity of substance.*" Masses of one kind of substance may be compared together by ascertaining the numbers of equal parts

which they contain; masses of substances of different kinds are compared by means to be afterwards referred to.

Accident.

The term "*accident*" will be applied to every variable state of substances, whether consisting in a condition of each part of a substance, how small soever, (which may be called an *absolute accident*), or in a physical relation between parts of substances, (which may be called a *relative accident*). Accidents to be the subject of scientific inquiry, must be capable of being measured and expressed by means of quantities. The quantity, even of an absolute accident, can only be expressed by means of a mentally-conceived relation.

The whole condition or state of a substance, so far as it is variable, is a *complex accident;* the independent quantities which are at once *necessary* and *sufficient* to express completely this complex accident, are *independent accidents*. To express the same complex accident, different systems of independent accidents may be employed; but the number of independent accidents in each system will be the same.

Examples.—The variable thermic condition of an elastic fluid is a *complex accident*, capable of being completely expressed by *two independent accidents*, which may be any two out of these three quantities—the *temperature*, the *density*, the *pressure*—or any two independent functions of these quantities.

The condition of strain at a point in an elastic solid, is a *complex accident*, capable of being completely expressed by *six independent accidents*, which may be the three elongations of the dimensions, and the three distortions of the faces of a molecule originally cubical, or the lengths and directions of the axes of the ellipsoidal figure assumed by a molecule originally spherical; or any six independent functions of either of those systems of quantities.

The distinction of accidents into absolute and relative is to a certain extent arbitrary; thus, the figure and dimensions of a molecule may be regarded as absolute accidents, when it is considered as a whole, or as relative accidents, when it is considered as made up of parts. Most kinds of accidents are necessarily relative, but some kinds can only be considered as relative accidents when some hypothesis is adopted as to the occult condition of the substances which they affect, as when heat is ascribed hypothetically to molecular motions; and such suppositions are excluded from the present inquiry.

Accidents may be said to be *homogeneous* when the quantities expressing them are capable of being put together, so that the result of the combination of the different accidents shall be expressed by one quantity. The number of heterogeneous kinds of accidents is evidently indefinite.

Effort, or Active Accident.

The term "*effort*" will be applied to every cause which varies, or tends to vary, an accident. This term, therefore, comprehends not merely *forces* or *pressures*, to which it is usually applied, but all causes of variation in the condition of substances.

Efforts may be homogeneous or heterogeneous.

Homogeneous efforts are compared by balancing them against each other.

An effort, being a condition of the parts of a substance, or a relation between substances, is itself an accident, and may be distinguished as an "*active accident.*"

With reference to a given limited substance, *internal efforts* are those which consist in actions amongst its parts; *external efforts* those which consist in actions between the given substance and other substances.

Passive Accident.

The condition which an effort tends to vary may be called a "*passive accident,*" and when the word "accident" is not otherwise qualified, "passive accident" may be understood.

Radical Accident.

If there be a quantity such that it expresses at once the magnitude of the passive accident caused by a given effort, and the magnitude of the active accident or effort itself, let the condition denoted by that quantity be called a "*radical accident.*"

[The velocity of a given mass is an example of a radical accident, for it is itself a passive accident, and also the measure of the kind of effort called accelerative force, which acting for unity of time, is capable of producing that passive accident.]

[The strength of an electric current is also a radical accident.]

Effort as a Measure of Mass.

Masses, whether homogeneous or heterogeneous, may be compared by means of the efforts required to produce in them variations of some particular accident. The accident conventionally employed for this purpose is *velocity*.

Work.

"*Work*" is the variation of an accident by an effort, and is a term comprehending all phenomena in which physical change takes place. *Quantity of work* is measured by the product of the variation of the passive accident by the magnitude of the effort, when this is constant;

or by the integral of the effort, with respect to the passive accident, when the effort is variable.

Let x denote a passive accident.

X an effort tending to vary it.

W the work performed in increasing x from x_0 to x_1, then,

$$(1.)\quad \begin{cases} W = \int_{x_0}^{x_1} X dx, \text{ and} \\ W = X\,(x_1 - x_0) \text{ if X is constant.} \end{cases}$$

Work is represented geometrically by the area of a curve, whereof the abscissa represents the passive accident, and the ordinate, the effort.

Energy, Actual and Potential.

The term "*energy*" comprehends every state of a substance which constitutes a capacity for performing work. *Quantities of energy* are measured by the quantities of work which they constitute the means of performing.

"*Actual energy*" comprehends those kinds of capacity for performing work which consist in particular states of each part of a substance, how small soever; that is, in an *absolute accident*, such as heat, light, electric current, *vis-viva*. Actual energy is essentially positive.

"*Potential energy*" comprehends those kinds of capacity for performing work which consist in relations between substances, or parts of substances; that is, in *relative accidents*. To constitute potential energy there must be a *passive accident* capable of variation, and an *effort* tending to produce such variation; the integral of this effort, with respect to the *possible variation* of the passive accident, is *potential energy*, which differs in work from this—that in work the change *has been effected*, which, in potential energy, is *capable of being effected.*

Let x denote an accident, x_1 its actual value; X, an effort tending to vary it; x_0, the value to which the effort tends to bring the accident; then

$$\int_{x_1}^{x_0} X dx = U, \text{ denotes potential energy.}$$

Examples of potential energy are, the chemical affinity of uncombined elements; the energy of gravitation, of magnetism, of electrical attraction and repulsion, of electro-motive force, of that part of elasticity which arises from actions between the parts of a body, and generally, of all mutual actions of bodies, and parts of bodies.

Potential energy may be passive or negative, according as the effort in question is of the same sign with the variation of the passive accident,

or of the opposite sign: that is, according as X is of the same sign with dx, or of the opposite sign.

It is to be observed, that the states of substances comprehended under the term *actual energy*, may possess the characteristics of potential energy also; that is to say, may be accompanied by a tendency or effort to vary relative accidents: as heat in an elastic fluid, is accompanied by a tendency to expand: that is, an effort to increase the volume of the receptacle containing the fluid.

The states to which the term *potential energy* is especially applied, are those which are solely due to mutual actions.

To put a substance into a state of energy, or to increase its energy, is obviously a *kind of work*.

IX. First Axiom.

All kinds of Work and Energy are Homogeneous.

This axiom means, that *any kind of energy may be made the means of performing any kind of work*. It is a fact arrived at by induction from experiment and observation, and its establishment is more especially due to the experiments of M. Joule.

This axiom leads, in many respects, to the same consequences with the hypothesis that all those kinds of energy which are not sensibly the results of motion and motive force are the results of occult modifications of motion and motive force.

But the axiom differs from the hypothesis in this, that the axiom is simply the generalized allegation of the facts proved by experience, while the hypothesis involves conjectures as to objects and phenomena which never can be subjected to observation.

It is the truth of this axiom which renders a science of energetics possible.

The efforts and passive accidents to which the branches of physics relate are varied and heterogeneous; but they are all connected with *energy*, a uniform species of quantity, which pervades every branch of physics.

This axiom is also equivalent to saying, that *energy is transformable and transferable* (an allegation which, in the previous paper referred to, was included in the definition of energy); for, to *transform energy*, means to employ energy depending on accidents of one kind, in putting a substance into a state of energy depending on accidents of another kind; and to *transfer energy*, means to employ the energy of one substance in putting another substance into a state of energy, both of which are kinds of work, and may, according to the axiom, be performed by means of any kind of energy.

X. Second Axiom.

The Total Energy of a Substance Cannot be Altered by the Mutual Actions of its Parts.

Of the truth of this axiom there can be no doubt; but some difference of opinion may exist as to the evidence on which it rests. There is ample experimental evidence from which it might be proved; but independently of such evidence, there is the argument, that the law expressed by this axiom is essential to the stability of the universe, such as it exists.

The special application of this law to mechanics is expressed in two ways, which are virtually equivalent to each other; the principle of *vis-viva*, and that of the equality of action and reaction. The latter principle is demonstrated by Newton, from considerations connected with the stability of the universe (*Principia*, Scholium to the Laws of Motion); for he shows, that but for the equality of action and reaction, the earth, with a continually accelerated velocity, would fly away through infinite space.

It follows, from the Second Axiom, that *all work consists in the transfer and transformation of energy alone;* for otherwise the total amount of energy would be altered. Also, that the energy of a substance can be varied by *external efforts alone.*

XI. External Potential Equilibrium.

The entire condition of a substance, so far as it is variable, as explained in Article VIII., under the head of *accident*, is a complex accident, which may be expressed in various ways by means of different systems of quantities denoting independent accidents; but the number of independent accidents in each system must be the same.

The quantity of work required to produce any change in the condition of the substance, that is to say, the potential energy received by it from without, during that change, may in like manner be expressed in different ways by the sums of different systems of integrals of external efforts, each integrated with respect to the independent accident which it tends to augment; but the number of integrals in each system, and the number of efforts, like the number of independent accidents, must be the same; and so also must the sums of the integrals, each sum representing the same quantity of work in a different way.

The different systems of efforts which correspond to different systems of independent accidents, each expressing the same complex accident, may be called *equivalent systems of efforts;* and the finding of a system of efforts equivalent to another may be called conversion of efforts.*

* The conversion of efforts in Physics, is connected with the theory of lineal transformations in Algebra.

When the law of variation of potential energy, by a change of condition of a substance, is known, the system of external efforts corresponding to any system of independent accidents is found by means of this principle:

Each effort is equal to the rate of variation of the potential energy with respect to the independent accident which that effort tends to vary: or symbolically

$$X = \frac{dU}{dx}. \qquad (2.)$$

External Potential Equilibrium *of a substance takes place, when the external effort to vary each of the independent accidents is null:* that is to say, *when the rate of variation of the potential energy of the substance with the variation of each independent accident is null.*

For a given substance, there are as many conditions of equilibrium, of the form

$$\frac{dU}{dx} = 0, \qquad (3.)$$

as there are independent accidents in the expression of its condition.

The special application of this law to motion and motive force constitutes the *principle* of *virtual velocities*, from which the whole science of statics is deducible.

XII. Internal Potential Equilibrium.

The internal potential equilibrium of a substance consists in the equilibrium of each of its parts, considered separately; that is to say, in the nullity of the rate of variation of the potential energy of each part with respect to each of the independent accidents on which the condition of such part depends.

Examples of particular cases of this principle are, the laws of the equilibrium of elastic solids, and of the distribution of statical electricity.

XIII. Third Axiom.

The Effort to Perform Work of a Given Kind, Caused by a Given Quantity of Actual Energy, is the Sum of the Efforts Caused by the Parts of that Quantity.

A law equivalent to this axiom, under the name of the "General Law of the Transformation of Energy," formed the principal subject of the previous paper already referred to.

This axiom appears to be a consequence of the definition of actual energy, as a capacity for performing work possessed by each part of a

substance independently of its relations to other parts, rather than an independent proposition.

Its applicability to natural phenomena arises from the fact, that there are states of substances corresponding to the definition of actual energy.

The mode of applying this third axiom is as follows:—

Let a homogeneous substance possess a quantity Q, of a particular kind of actual energy, uniformly distributed, and let it be required to determine the amount of the effort arising from the actual energy, which tends to perform a particular kind of work W, by the variation of a particular passive accident x.

The total effort to perform this kind of work is represented by the rate of its increase relatively to the passive accident, viz.,—

$$X = \frac{dW}{dx}$$

Divide the quantity of actual energy Q into an indefinite number of indefinitely small parts δQ; the portion of the effort X due to each of those parts will be

$$\delta Q \frac{dX}{dQ}$$

and adding these partial efforts together, the effort caused by the whole quantity of actual energy will be

$$(4.) \qquad Q \frac{dX}{dQ} = Q \frac{d^2W}{dQ\,dx}$$

If this be equal to the *effective effort* X, then that effort is simply proportional to, and wholly caused by, the actual energy Q. This is the case of the pressure of a perfect gas, and the centrifugal force of a moving body.

If the effort caused by the actual energy differs from the effective effort, their difference represents, when the former is the less, an additional effort

$$(5.) \quad \left(1 - Q\frac{d}{dQ}\right) X,$$

and when the former is the greater, a counter effort

$$\left(Q\frac{d}{dQ} - 1\right) X,$$

due to some other cause or causes.

XIV. Rate of Transformation; Metamorphic Function.

The effort to augment a given accident x, caused by actual energy of a given kind Q, may also be called the "*Rate of Transformation*" of the

given kind of actual energy with increase of the given accident; for the limit of the amount of actual energy which disappears in performing work by an indefinitely small augmentation dx, of the accident, is

$$(6.) \qquad \begin{aligned} d\mathrm{H} &= \mathrm{Q}\,\frac{d\mathrm{X}}{a\mathrm{Q}}\,dx \\ &= \mathrm{Q}\,\frac{d^2\mathrm{W}}{d\mathrm{Q}\,dx}\,dx = \mathrm{Q}d\,\frac{d\mathrm{W}}{d\mathrm{Q}} \end{aligned}$$

The *last* form of the above expression is obviously applicable when the work W is the result of the variation of any number of independent accidents, each by the corresponding effort. For example, let x, y, z, &c., be any number of independent accidents, and X, Y, Z, &c., the efforts to augment them; so that

$$d\mathrm{W} = \mathrm{X}dx + \mathrm{Y}dy + \mathrm{Z}dz + \&\mathrm{c}.$$

Then

$$(7.) \qquad \begin{aligned} d\mathrm{H} &= \mathrm{Q}\left\{\frac{d\mathrm{X}}{d\mathrm{Q}}\,dx + \frac{d\mathrm{Y}}{d\mathrm{Q}}\,dy + \frac{d\mathrm{Z}}{d\mathrm{Q}}\,dz + \&\mathrm{c}.\right\} \\ &= \mathrm{Q}d\,\frac{d\mathrm{W}}{d\mathrm{Q}} \quad \text{as before.} \end{aligned}$$

The function of actual energy, efforts, and passive accidents, denoted by

$$(8.) \qquad \frac{d\mathrm{W}}{d\mathrm{Q}} = \int\frac{d\mathrm{H}}{\mathrm{Q}} = \mathrm{F},$$

whose variation, multiplied by the actual energy, gives the amount of actual energy transformed in performing the work $d\,\mathrm{W}$, may be called the "METAMORPHIC FUNCTION" of the kind of actual energy Q relatively to the kind of work W.

When this metamorphic function is known for a given homogeneous substance, the quantity H of actual energy of the kind Q transformed to the kind of work W, during a given operation, is found by taking the integral

$$(9.) \qquad \mathrm{H} = \int \mathrm{Q}\,d\,\mathrm{F}.$$

The transformation of actual energy into work by the variation of passive accidents is a *reversible operation;* that is to say, if the passive accidents be made to vary to an equal extent in an opposite direction, potential energy will be exerted upon the substance, and transformed into actual energy: a case represented by the expression (9.) becoming negative.

The metamorphic function of heat relatively to expansive power, was first employed in a paper on the Economy of Heat in Expansive Ma-

chines, read to the Royal Society of Edinburgh in April 1851 ("Trans. Roy. Soc. Edin.," vol. xxi.)

The metamorphic function of heat relatively to electricity was employed by Professor William Thomson, in a paper on Thermo-Electricity, read to the Royal Society of Edinburgh in May 1854 ("Trans. Roy. Soc. Edin.," vol. xxi.), and was the means of anticipating some most remarkable laws, afterwards confirmed by experiment.

XV. Equilibrium of Actual Energy; Metabatic Function.

It is known by experiment, that a state of actual energy is directly transferable; that is to say, the actual energy of a particular kind (such as heat), in one substance, may be diminished, the sole work performed being an equal augmentation of the same kind of actual energy in another substance.

Equilibrium of actual energy of a particular kind Q between substances A and B, takes place, when the tendency of B to transfer this kind of energy to B is equal to the tendency of B to transfer the same kind of energy to A.

Laws respecting the equilibrium of particular kinds of actual energy have been ascertained by experiment, and in some cases anticipated by means of mechanical hypotheses, according to which, all actual energy consists in the *vis-viva* of motion.

The following law will now be proved, respecting the equilibrium of actual energy of all possible kinds:—

Theorem.—If equilibrium of actual energy of a given kind take place between a given pair of substances, possessing respectively quantities of actual energy of that kind in a given ratio, then that equilibrium will subsist for every pair of quantities of actual energy bearing to each other the same ratio.

Demonstration.—The tendency of one substance to transfer actual energy of the kind Q to another, must depend on some sort of effort, whose nature and laws may be known or unknown. Let Y_A be this effort for the substance A, Y_B the corresponding effort for the substance B. Then a condition of equilibrium of actual energy is

$$Y_A = Y_B \tag{10.}$$

The effort Y may or may not be proportionate to the actual energy Q multiplied by a quantity independent of Q.

Case first.—If it is so proportional, let

$$Y = \frac{1}{K} Q,$$

K being independent of Q; then the condition of equilibrium becomes

$$\frac{1}{K_A} Q_A = \frac{1}{K_B} Q_B, \text{ or}$$

$$\frac{Q_B}{Q_A} = \frac{K_B}{K_A},$$

a ratio independent of the absolute amounts of actual energy.

Case second.—If the effort Y is not simply proportional to the actual energy Q, the portion of it caused by that actual energy, according to the principle of article 13, deduced from the third axiom, is, for each substance,

$$Q \frac{dY}{dQ}$$

and a second condition of equilibrium of actual energy is furnished by the equation

$$(11.) \qquad Q_A \frac{dY}{dQ_A} = Q_B \frac{dY_B}{dQ_B}$$

In order that this condition may be fulfilled simultaneously with the condition (10.) it is necessary that

$$\frac{dQ_A}{Q_A} = \frac{dQ_B}{Q_B}$$

that is to say, that the ratio of the quantities of actual energy in the two substances should be independent of those quantities themselves; a condition expressed, as before, by

$$(11.) \qquad \frac{Q_B}{Q_A} = \frac{K_B}{K_A}$$

Q.E.D.

This ratio is a quantity to be ascertained by experiment, and may be called the ratio of the SPECIFIC ACTUAL ENERGIES of the substances A and B, for the kind of energy under consideration.

The function

$$(12.) \qquad \frac{Q_A}{K_A} = \frac{Q_B}{K_B} = \theta$$

whose identity for the two substances expresses the condition of equilibrium of the actual energy Q between them, may be called the "METABATIC FUNCTION" for that kind of energy.

In the science of thermo-dynamics, the metabatic function is ***absolute temperature;*** and the factor K is *real specific heat.* The theorem stated above, when applied to heat, amounts to this: *that the real specific heat of a substance is independent of its temperature.*

XVI.—Use of the Metabatic Function; Transformation of Energy in an aggregate.

From the mutual proportionality of the actual energy Q, and the metabatic function θ, it follows that the operations

$$Q\frac{d}{dQ},\ \theta\frac{d}{d\theta}$$

are equivalent; and that the latter may be substituted for the former in all the equations expressing the laws of the transformation of energy. We have therefore

$$(13.)\qquad Q\frac{dX}{dQ}=\theta\frac{dX}{d\theta}=\theta\frac{d^2W}{d\theta dx}$$

for the effort to transform actual energy of the kind Q into work of the kind W, when expressed in terms of the metabatic function; and

$$(14.)\qquad dH=\theta d\frac{dW}{d\theta}$$

for the limit of the indefinitely small transformation produced by an indefinitely small variation of the accidents on which the kind of work W depends.

There is also a form of *metamorphic function.*

$$(15.)\qquad \varphi=\frac{dW}{d\theta}=\int\frac{dH}{\theta}=KF$$

suited for employment along with the metabatic function, in order to find, by the integration

$$(16.)\qquad H=\int\theta\, d\varphi$$

the quantity of actual energy of a given kind Q transformed to the kind of work W during any finite variation of accidents.

The advantage of the above expressions is, that they are applicable, not merely to a homogeneous substance, but to any *heterogeneous substance or aggregate,* which is internally in a state of equilibrium of actual and potential energy; for throughout all the parts of an aggregate in that condition, the metabatic function θ is the same, and each of the efforts X, &c., is the same, and consequently the metamorphic function φ is the same.

"*Carnôt's function*" in thermo-dynamics is proportional to the reciprocal of the metabatic function of heat.

XVII. Efficiency of Engines.

An engine is a contrivance for transforming energy by means of the periodical repetition of a cycle of variations of the accidents of a substance.

The *efficiency* of an engine is the proportion which the energy permanently transformed to a useful form by it bears to the whole energy communicated to the working substance.

In a *perfect engine* the cycle of variations is thus:—

I. The metabatic function is increased, say from θ_0 to θ_1.

II. The metamorphic function is increased by the amount $\Delta\varphi$.

III. The metabatic function is diminished from θ_1 back to θ_0.

IV. The metamorphic function is diminished by the amount $\Delta\varphi$.

During the second operation, the energy received by the working substance, and transformed from the actual to the potential form is $\theta_1 \Delta\varphi$. During the fourth operation energy is transformed back, to the amount $\theta_0 \Delta\varphi$. So that the energy permanently transformed during each cycle is $(\theta_1 - \theta_0)\Delta\varphi$; and the efficiency of the engine $\frac{\theta_1 - \theta_0}{\theta_1}$

XVIII. Diffusion of Actual Energy; Irreversible or Frictional Operations.

There is a tendency in every substance or system of substances, to the *equable diffusion* of actual energy; that is to say, to its transfer between the parts of the substance or system, until the value of the *metabatic function* becomes uniform.

This process is *not directly reversible;* that is to say, there is no such operation as a direct concentration of actual energy through a tendency of the metabatic function to become unequal in different parts of a substance or system.

Hence arises the impossibility of using the energy re-converted to the actual form at the lower limit of the metabatic function in an engine.

There is an analogy in respect of this property of *irreversibility*, between the diffusion of one kind of actual energy, and certain irreversible transformations of one kind of actual energy to another, called by Professor William Thomson, "Frictional phenomena," viz., the production of heat by rubbing and agitation, and by electric currents in a homogeneous substance at a uniform temperature.

In fact, a conjecture may be hazarded, that immediate diffusion of the actual energy produced in frictional phenomena, is the circumstance which renders them irreversible; for, suppose a small part of a substance to have its actual energy increased by the exertion of some kind of work

upon it; then, if the increase of actual energy so produced be immediately diffused amongst other parts, so as to restore the uniformity of the metabatic function, the whole process will be irreversible. This speculation, however, is, for the present, partly hypothetical; and, therefore does not, strictly speaking, form part of the science of energetics.

XIX. Measurement of Time.

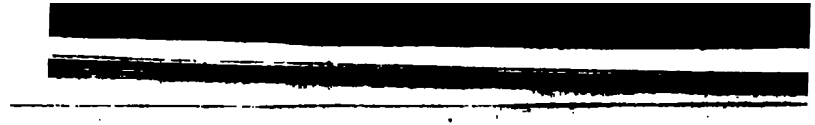
The general relations between energy and time must form an important branch of the science of energetics; but for the present, all that I am prepared to state on this subject is the following DEFINITION OF EQUAL TIMES:—

Equal times are the times in which equal quantities of the same kind of work are performed by equal and similar substances, under wholly similar circumstances.

XX. Concluding Remarks.

It is to be observed, that the preceding articles are not the results of a new and hitherto untried speculation, but are the generalized expression of a method of reasoning which has already been applied with success to special branches of physics.

In this brief essay, it has not been attempted to do more than to give an outline of some of the more obvious principles of the science of energetics, or the abstract theory of physical phenomena in general; a science to which the maxim, true of all science, is specially applicable—that its subjects are boundless, and that they never can, by human labours, be exhausted, nor the science brought to perfection.





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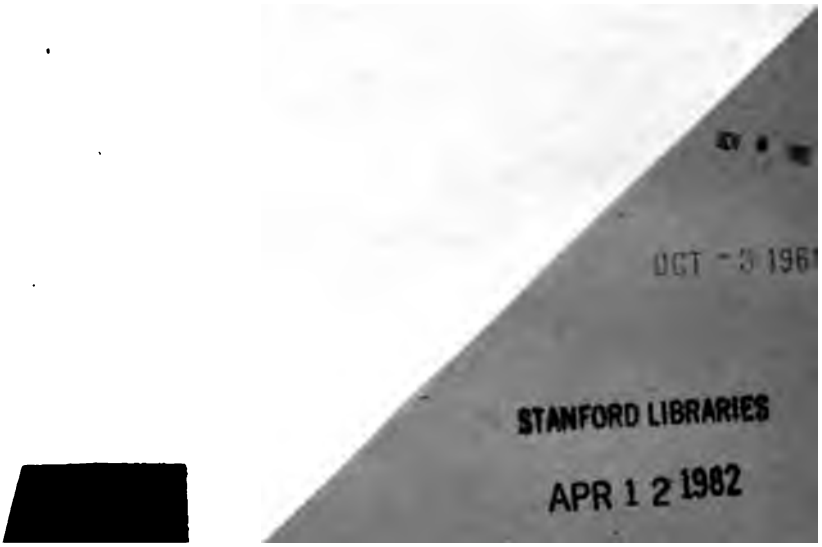



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